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PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From November 15, 1894, *to March* 21, 1895

VOL. LVII.

LONDON:
HARRISON AND SONS, ST. MARTIN'S LANE,
Printers in Ordinary to Her Majesty.
MDCCCXCV.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

ERRATA FOR 'PROCEEDINGS,' VOL. 56, MAY, 1894.

In 8th line from top of page 92, § 10, *for* "no" *substitute* "comparatively little."

In last line but one of § 13, page 93, *for* "6.10^{-5}" *substitute* "6.10^{-11}," and *for* "4·8 per cent." *substitute* "$4{\cdot}8.10^{-8}$."

Delete the first paragraph of § 14.

In line 5 from the end of § 14, *for* "Even" *substitute* "But," and in line 3 from the end of § 14 *for* "sufficient" *substitute* "insufficient."

I. "Further Observations on the Organisation of the Fossil Plants of the Coal-Measures. Part 2. The Roots of *Calamites*." By W. C. WILLIAMSON, LL.D., F.R.S., Emeritus Professor of Botany in the Owens College, Manchester; and D. H. SCOTT, M.A., Ph.D., F.R.S., Honorary Keeper of the Jodrell Laboratory, Royal Gardens, Kew. Received October 31, 1894.

(Abstract.)

Until quite recently our knowledge of the adventitious roots of *Calamites* was limited to their external form and position on the stem. Though the structure of the stem itself was so well known, we remained in complete ignorance of the internal organisation of the roots.

M. Renault, in 1885, expressed the opinion that the fossils originally described by Dr. Williamson under the name of *Astromyelon*,* in which the structure is well-preserved, were the roots of *Arthropitus* (our *Calamites*) and *Calamodendron*. He has now found conclusive proof of the truth of this hypothesis, and in his latest work† he gives numerous figures of roots, with *Astromyelon* structure, arising as appendages on the stems of *Calamites*, *Bornia*, and *Calamodendron*.

It therefore became necessary to reinvestigate our English specimens of *Astromyelon Williamsonis* (from which the French fossils are said to be specifically distinct) in order to ascertain their true nature.

A specimen in the Williamson collection shows clearly three appendages with the typical structure of *Astromyelon*, arising from a main axis which shows structural features characteristic of a Calamitean stem. M. Renault's conclusion may, therefore, safely be extended to the English specimens.

The structure of the fossils hitherto known as *Astromyelon Williamsonis* has been examined in detail, with a view to determining their morphological nature. It is found that they present, in all respects, the characteristics of *roots*, so far as these can be recognised in fossil plants.

In the larger specimens, such as have been found in direct connection with the stem of *Calamites*, there is a well-marked medulla, which is surrounded by a ring of primary xylem-strands, varying in number, from twenty-five downwards. The development of each xylem-strand was centripetal, for the spiral tracheides lie at the external angle of each strand.

In well-preserved specimens, at an early stage of development, the primary phloëm can be recognised, alternating with the primary strands of wood. These anatomical features are characteristic of the roots of all vascular plants.

The secondary wood, which has been observed at all stages of development, agrees in structure with that of the stem of *Calamites*. In favourable cases the cambium can be seen, and secondary phloëm has also been found.

The mode of branching is shown to be *endogenous*.

There is some evidence that these organs had a double endodermis, as is the case in the roots of the recent *Equiseta*.

At the exterior of the cortex (which always had a lacunar zone) a protective epidermoidal layer was formed, probably arising from the cells next below the actual epidermis. This feature occurs in many recent roots.

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* See Williamson, "Organisation of Fossil Plants of Coal-Measures, Parts IX and XII," 'Phil. Trans.,' 1878 and 1883; Renault, "Genre Astromyelon," 'Ann. des Sci. Géologiques,' vol. 17, 1885, and 'Mém. de la Soc. des Sci. Nat. de Saône et Loire,' 1885.

† 'Flore Fossile d'Autun,' &c. Part 2. 1893.

It is shown incidentally that the doubts expressed by Messrs. Hick and Cash as to the identification of their "*Myriophylloides*" with "*Astromyelon*" are unfounded. A section of the type-specimen of "*Myriophylloides*" has been re-examined, and its structure is shown to be identical with that of the other specimens of "*Astromyelon*."

The numerous minute rootlets, associated with the larger roots, have been carefully examined. Many of these rootlets are without any pith, but they are in other respects identical with the typical specimens, with which they are connected by an unbroken series of intermediate forms. There are also instances in which rootlets are found inserted upon the medullate roots.

The conclusions at which the authors arrive are the following:—

1. The fossils hitherto described under the name of *Astromyelon Williamsonis* are the adventitious roots of *Calamites.*

2. Their structure is in all respects that characteristic of roots, as is proved by the centripetal primary wood, the alternating strands of primary wood and phloëm, the endogenous mode of branching, and the absence of nodes.

3. The smallest specimens, with little or no medulla, represent the finest branches of the same roots, of which the large medullate forms are the relatively main axes.

The paper is illustrated by micro-photographs from the actual specimens, and also by *camera-lucida* drawings.

II. "On the Ascent of Sap." By HENRY H. DIXON, B.A. Assistant to the Professor of Botany, Trinity College, Dublin, and J. JOLY, M.A., Sc.D., F.R.S. Received October 16, 1894.

(Abstract.)

Strasburger's experiments have eliminated the direct action of living protoplasm from the problem of the ascent of sap, and have left only the tracheal tissue, as an organised structure, and the transpiration-activity of the leaf, wherein to seek an explanation of the phenomenon. The authors investigate the capability of the leaf to transpire against excessive atmospheric pressures. In these experiments the leaf was found able to bring forward its water meniscuses against the highest pressures attained and freely transpire. Whether the draught upon the sap established at the leaf during transpiration be regarded as purely capillary or not, these experiments lead the authors to believe that it alone is quite adequate to effect the elevation by direct tension of the sap in tall trees. Explanations of the lifting of the sap from other causes prove inadequate.

A reconsideration of the principal experiments of previous observers and some new experiments of the authors lead to the view that the ascent is principally in the lumen and not in the wall.

The explanation of how the tensile stress is transmitted in the ascending sap without rupture of the column of liquid is found in the stable condition of this liquid. The state of stability arises from two circumstances:—the internal stability of a liquid when mechanically stretched, whether containing dissolved gases or not, and the additional stability conferred by the minutely subdivided structure of the conducting tissue, which renders the stressed liquid stable even in the presence of free gas.

By direct experiments upon water containing large quantities of dissolved air, the state of internal stability is investigated. And, further, by sealing up in the vessels, in which the water to be put under tension is contained, chips of the wood of *Taxus baccata*, the authors find that their presence in no case gives rise to rupture of the stressed liquid, but that this occurs preferably anywhere else, and usually on the glass walls. The establishment of tensile stress is effected in the usual way, by cooling the completely filled vessel. A measurement possessing considerable accuracy afforded 7½ atmospheres as being attained in some of the experiments.

The second condition of stability arises directly from the property of the pit-membranes to oppose the passage of free gas, while they are freely permeable to the motion of a liquid. Hence a chance development of free gas is confined in effect to the minute dimensions of the compartment in which it is evolved, and this one lumen alone is rendered for the time being non-conducting. On the other hand, in the water-filled portion of the tracheal tissue, the closing membranes, occupying the median and least obstructive position, the motion of the stressed sap is freely allowed. The structure of the conducting tissue is, in fact, a configuration conferring stability on a stressed liquid in the presence (from various causes) of free gas. As neither free gas nor unwetted dust particles can ascend with the sap, the authors contend that the state of tensile stress necessary to their hypothesis is inevitably induced.

The energy relations of the leaf with its surroundings, on the assumption that evaporation at capillary water-surfaces is mainly responsible for the elevation of sap, may be illustrated by the well-known power of the water-filled porous pot to draw up mercury in a tube to which it is sealed. The authors describe an engine in which the energy entering in the form of heat at the capillary surfaces may be in part utilised to do mechanical work: a battery of twelve small porous pots, freely exposed to the air, keeping up the continuous rotation of a fly-wheel. Replacing the porous pots by a transpiring branch, this too maintains the wheel in rotation. This is, in fact, a vegetable engine. In short, the transpiration effects going on at the leaf are, in so far as they are the result of spontaneous evaporation and uninfluenced by other physiological phenomena, of the "sorting demon" class, in which the evaporating surface plays the part of a sink of thermal energy.

If the tensile stress in the sap is transmitted to the root, the authors suggest that this will establish in the capillaries of the root-surface meniscuses competent to condense water rapidly from the surrounding soil. They show by experiment the power possessed even by a root injured by lifting from the soil, of condensing water vapour from a damp atmosphere. Such a state of things may be illustrated by a system (which the authors realised) consisting of two porous pots connected by a tube and all filled with water; one, the "leaf," exposed to the air gives out vapour, the other, the "root," buried in damp earth supplies the demand of the "leaf," and an upward current in the connecting tube is established.

III. "The Pigments of the Pieridæ. A Contribution to the Study of Excretory Substances which function in Ornament." By F. GOWLAND HOPKINS, Demonstrator of Physiology and Chemistry at Guy's Hospital, London. Communicated by Professor E. RAY LANKESTER, F.R.S. Received October 5. 1894.

(Abstract.)

The paper deals with the chemistry of the wing pigments of that group of butterflies known as the Pieridæ, and demonstrates the excretory nature of these pigments. The following are the salient facts dealt with, most of the statements being based on original observations described in the paper:—

1. The wing scales of the white Pieridæ are shown to contain *uric acid*, this substance bearing the same relation to the scale as do the pigments in the coloured Pieridæ, and therefore functioning practically as a white pigment.

2. The yellow pigment which is so widely distributed in the Pieridæ (being found in the majority of the genera) is shown to be a derivative of uric acid.

3. The properties of this yellow pigment are described, and the results of its analysis are given. The pigments of various yellow-coloured genera are shown to be identical.

4. It is shown that this yellow pigment may be artificially produced by heating uric acid with water in sealed tubes at high tem-

peratures. The product so obtained was originally described by Hlasiwetz as "mycomelic acid"; but evidence is brought forward to show that the substance described and analysed by this chemist was in reality urate of ammonium coloured by a yellow body, probably identical with the natural pigment.

5. The identity of the natural and artificial products is demonstrated by the fact that both yield under like treatment a purple derivative, which has a well-marked and easily identified absorption spectrum.

6. The artificial yellow product has not yet been obtained in a pure condition, but it may be so far purified as to exhibit clearly all the general properties of the natural pigment.

7. The natural pigment as prepared for analysis is shown to be almost certainly a chemical individual. Its probable constitution is discussed.

8. It is shown that this yellow substance (denominated in the paper "Lepidotic acid"), together with a closely allied red substance, will account for all the chemical pigmentation of the wing scales of the coloured Pieridæ, though modifications may be produced by superadded optical effects. The black pigment found in the group is not dealt with in the paper.

9. The described uric acid derivatives, though universal in the Pieridæ, are apparently confined to this group among the Rhopalocera. This fact enables the interesting observation to be made, that where a Pierid mimics an insect belonging to another family, the pigments in the two cases are chemically quite distinct. This is well seen in the genera *Leptalis* and *Mechanitis* respectively.

10. The existence of pigments other than scale-pigments is for the first time described; substances, namely, which are found between the wing membranes, and which, in certain genera, are the basis of ornament.

11. The fact that the scale-pigments are really the normal excretory products of the animal utilised in ornament, is emphasised by the observation that the yellow Pierids, on emergence from the chrysalis, are apt to void from the rectum a quantity of uric acid coloured by a yellow substance which exactly resembles the pigment of the wing.

Presents, November 15, 1894.

Transactions.

Amsterdam:—K. Akademie van Wetenschappen. Verhandelingen. Deel II. Nos. 1—6, 8. Deel III. 8vo. *Amsterdam* 1893–94; Jaarboek. 1893. 8vo. *Amsterdam;* Verslagen der Zittingen. 1893–94. 8vo. *Amsterdam.* The Academy.

Transactions (*continued*).

Baltimore:—Johns Hopkins University. Circulars. Vol. XIII. Nos. 113—114. 4to. *Baltimore* 1894; Studies in Historical and Political Science. Series XII. Nos. 5—9. 8vo. *Baltimore* 1894. The University.

Medical and Chirurgical Faculty of the State of Maryland. Transactions. Session 96. 8vo. *Baltimore* 1894. The Faculty.

Peabody Institute. Annual Report. 1894. 8vo. *Baltimore.* The Institute.

Bangor:—University College of North Wales. Calendar. 1894-95. 8vo. *Manchester* 1894. The College.

Basel:—Naturforschende Gesellschaft. Verhandlungen. Bd. IX. Heft 3. 8vo. *Basel* 1893. The Society.

Belgrade:—Royal Servian Academy. Spomenik. 23—24. 4to. *Belgrade* 1894; Glas. 43—44. 8vo. *Belgrade* 1894; Godishnak. 5—7. 8vo. *Belgrade* 1892-94. The Academy.

Berkeley:—University of California. Bulletin of the Department of Geology. Vol. I. Nos. 3—4. 8vo. *Berkeley* 1893; Report of Work of the Agricultural Experiment Stations. 1891-92. 8vo. *Sacramento* 1893. And three other University Publications. 8vo. The University.

Berlin:—Gesellschaft für Erdkunde. Verhandlungen. Bd. XXI. Nos. 6—8. 8vo. *Berlin* 1894; Zeitschrift. Bd. XXIX. Nos. 2—4. 8vo. *Berlin* 1894. The Society.

K. Akademie der Wissenschaften. Abhandlungen. 1893. 4to. *Berlin* 1893; Sitzungsberichte. 1894. Nos. 1—38. 8vo. *Berlin;* Die Behördenorganisation und die allgemeine Staatsverwaltung Preussens im 18. Jahrhundert. Bd. I. 4to. *Berlin* 1894. The Academy.

Physikalisch-Technische Reichsanstalt. Wissenschaftliche Abhandlungen. Bd. 1. 4to. *Berlin* 1894. The Institute.

Birmingham:—Mason College. Calendar. 1894-95. 8vo. *Birmingham* 1894. The College.

Bucharest:—Societăţii de Sciinţe Fizice. Buletinul. Anul III. Nos. 3—4. 8vo. *Bucurescĭ* 1894. The Society.

Calcutta:—Asiatic Society of Bengal. Journal. Vol. LXIII. Part 1. Nos. 1—2. Part 2. No. 1. Part 3. No. 1. 8vo. *Calcutta* 1894; Proceedings. 1894. Nos. 2—6. 8vo. *Calcutta* 1894. The Society.

Royal Botanic Garden. Annals. Vol. IV. 4to. *Calcutta* 1893. The Director.

Cambridge, Mass.:—Museum of Comparative Zoology. Bulletin. Vol. XXV. Nos. 7—8. 8vo. *Cambridge, Mass.* 1894. The Museum.

Transactions (*continued*).

Canada:—Royal Society of Canada. Proceedings and Transactions. Vol. XI. 4to. *Ottawa* 1894. The Society.

Cape Town:—South African Philosophical Society. Transactions. Vol. V. Part 2. Vol. VII. Part 1. Vol. VIII. Part 1. 8vo. *Cape Town* 1893. The Society.

Catania:—Accademia Gioenia di Scienze Naturali. Atti. Tomo XIV—XX. Serie 2. Tomo V. XVI. Serie 3. Tomo IV. 4to. *Catania* 1839–70. The Academy.

Chapel Hill:—Elisha Mitchell Scientific Society. Journal. Vol. X. Part 2. 8vo. *Chapel Hill, N.C.* 1894. The Society.

Charlottenburg:—Physikalisch-Technische Reichsanstalt. 5^ter^ Bericht über die Thätigkeit. 8vo. *Berlin* 1894. The Institute.

Copenhagen:—K.D. Videnskabernes Selskab. Oversigt. 1893. No. 3. 1894. No. 1. 8vo. Kjøbenhavn. The Society.

Cordova:—Academia Nacional de Ciencias. Boletin. Tomo XII. Entr. 1—4. Tomo XIII. Entr. 1—2. 8vo. *Buenos Aires* 1890–93. The Academy.

Cork:—Queen's College. Calendar. 1894–95. 8vo. *Cork* 1894. The College.

Cracow:—Académie des Sciences. Bulletin International. Mai —Juillet, 1894. 8vo. *Cracovie.* The Academy.

Dantzic:—Naturforschende Gesellschaft. Schriften. Bd. VIII. Heft 3—4. 8vo. *Danzig* 1894. The Society.

Dorpat:—Universität. Inaugural-Dissertationen. 1893–94. 8vo. The University.

Edinburgh:—Royal Physical Society. Proceedings. Sessions 1892–94. 8vo. *Edinburgh* 1893–94. The Society.

Royal Society. Transactions. Vol. XXXVII. Part 3. 4to. *Edinburgh* 1894; Proceedings. Vol. XX. Pp. 161—304. 8vo. [*Edinburgh*] 1894. The Society.

Emden:—Naturforschende Gesellschaft. Jahresbericht. 1892–93. 8vo. *Emden* 1894. The Society.

Erlangen:—Physikalisch-medicinische Societät. Sitzungsberichte. Heft 25. 8vo. *Erlangen* 1893. The Society.

Falmouth:—Royal Cornwall Polytechnic Society. Sixty-first Annual Report. 1893. 8vo. *Penryn.* The Society.

Frankfort-on-the-Main:—Senckenbergische Naturforschende Gesellschaft. Abhandlungen. Bd. XVIII. Heft 3. 4to. *Frankfurt a. M.* 1894; Bericht. 1894. 8vo. *Frankfurt a. M.* The Society.

Frankfort-on-the-Oder:—Naturwissenschaftlicher Verein. Helios. Jahrg. XII. Nos. 1—6. 8vo. *Frankfurt a. O.* 1894; Societatum Litteræ. Jahrg. VIII. Nos. 4—9. 8vo. *Frankfurt. a. O.* 1894. The Society.

Transactions (*continued*).

Geneva:—Institut National Genevois. Bulletin. Tome XXXII. 8vo. *Genève* 1894. The Institute.

Genoa:—Società Ligustica di Scienze Naturali e Geografiche. Atti. Vol. V. No. 2. 8vo. *Genova* 1894. The Society.

Giessen:—Universität. Inaugural-Dissertationen. 1893–94. 8vo. The University.

Gloucester:—Cotteswold Naturalists' Field Club. Proceedings. Vol. XI. Part 2. 8vo. *Gloucester* [1894]. The Club.

Götheborg:—Kongl. Vetenskaps och Vitterhets Samhälle. Handlingar. Häft. XXVI—XXIX. 8vo. *Göteborg* 1891–94. The Society.

Göttingen:—K. Gesellschaft der Wissenschaften. Abhandlungen (Math.-Phys. Klasse). Bd. XXXIX. 4to. *Göttingen* 1894; Abhandlungen (Hist.-Philol. Klasse). Bd. XXXIX. 4to. *Göttingen* 1894; Nachrichten (Math.-Phys. Klasse). 1894. Nos. 1—3. 8vo. *Göttingen;* Nachrichten (Philol.-Hist. Klasse). 1894. No. 2. 8vo. *Göttingen;* Protocolle der bei der Delegirtenconferenz in Innsbruck vom 6—7. September 1894 abgehaltenen Sitzungen. 8vo. [*Göttingen* 1894.] The Society.

Graz:—Naturwissenschaftlicher Verein für Steiermark. Mittheilungen. Jahrg. 1893. 8vo. *Graz* 1894. The Society.

Guatemala:—Sociedad Gualtemalteca de Ciencias. Revista Mensual. Tomo II. Núm. 6—9. 8vo. *Guatemala* 1894. The Society.

Hamburg:—Naturhistorisches Museum. Mitteilungen. Jahrg. XI. 8vo. *Hamburg* 1894. The Museum.

Hamilton:—Hamilton Association. Journal and Proceedings. No. 10. 8vo. *Hamilton* 1894. The Association.

Hermannstadt:—Siebenbürgischer Verein für Naturwissenschaften. Verhandlungen und Mittheilungen. Jahrg. XLIII. 8vo. *Hermannstadt* 1894. The Society.

Hobart:—Royal Society of Tasmania. Papers and Proceedings. 1893. 8vo. *Hobart* 1894. The Society.

Houghton:—Michigan Mining School. Catalogue. 1895–96. 8vo. *Houghton* 1894. The School.

Innsbruck:—Ferdinandeum. Zeitschrift. Heft 38. 8vo. *Innsbruck* 1893. The Ferdinandeum.

Naturwissenschaftlich-Medizinischer Verein. Bericht. Jahrg. XXI. 8vo. *Innsbruck* 1894. The Society.

Jena:—Medizinisch-naturwissenschaftliche Gesellschaft. Jenaische Zeitschrift. Bd. XXVIII. Heft 4. 8vo. *Jena* 1894. The Society.

Transactions (*continued*).

Karlsruhe:—Technische Hochschule. Inaugural-Dissertationen. 1893–94. 8vo. *Karlsruhe.* The School.

Kazan:—Imperial University. Scientific Notes. 1894. No. 4. [*Russian.*] 8vo. *Kazan.* The University.

Kew:—Royal Gardens. Bulletin of Miscellaneous Information. 1894. Nos. 90—94. 8vo. *London.* The Director.

Kiel:—Universität. Schriften. 1893–94. 8vo. The University.

Klausenburg:—Erdélyi Múzeum Egylet. Értesitő. Évf. XIX. Szak 2. Füzet 1—2. 8vo. *Kolozsvárt* 1894. The Society.

Königsberg:—Physikalisch-Ökonomische Gesellschaft. Schriften. Jahrg. XXXIV. 4to. *Königsberg* 1893. The Society.

Lansing:—Michigan Mining School. Reports of the Director. 1890–92. 8vo. *Lansing* 1893. The School.

Leipsic:—Astronomische Gesellschaft. Vierteljahrsschrift. Jahrg. XXIX. Heft 2. 8vo. *Leipzig* 1894. The Society.

K. Sächs. Gesellschaft der Wissenschaften. Abhandlungen (Math.-Phys. Classe). Bd. XXI. No 2. 8vo. *Leipzig* 1894; Abhandlungen (Phil.-Hist. Classe). Bd. XIV. No. 6. 8vo. *Leipzig* 1894. The Society.

Lisbon:—Academia Real das Sciencias. Sessão Publica em Dezembro de 1893. 8vo. *Lisboa* 1893; Documentos remettidos da India. Tomo IV. 4to. *Lisboa* 1893; [and 13 other publications. 8vo. and 4to.] The Academy.

London:—Anthropological Institute. Journal. Vol. XXIV. No. 1. 8vo. *London* 1894. The Institute.

British Astronomical Association. Memoirs. Vol. III. Parts 2—3. 8vo. *London* 1894; Journal. Vol. IV. Nos. 7—10. 8vo. *London* 1894. The Association.

British Museum. Catalogue of Printed Books. Luther. Pla—Plu. Plu—Pol. Pope—Por. Por—Pou. Pri—Pro. Pue—Pz. Q—Qwi. Folio. *London* 1894. The Trustees.

Camera Club. Journal. Vol. VIII. Nos. 98—102. 8vo. *London* 1894. The Club.

Entomological Society. Transactions. 1894. Part 3. 8vo. *London.* The Society.

Geological Society. Quarterly Journal. Vol. L. Parts 3—4. 8vo. *London* 1894; List. 1894. 8vo. *London.* The Society.

Geologists' Association. Proceedings. Vol. XIII. Parts 8—9. 8vo. *London* 1894. The Association.

Institute of Brewing. Transactions. Vol. VII. Nos. 7—8. 8vo. *London* 1894. The Institute.

Transactions (*continued*).

Institute of Chemistry. Register. 1894–95. 8vo. *London.* The Institute.

Institution of Civil Engineers. Minutes of Proceedings, Vol. CXVI. 8vo. *London* 1894; Brief Subject-Index. Vols. LIX—CXVIII. 8vo. *London* 1894. The Institution.

Institution of Mechanical Engineers. Proceedings. 1894. Nos. 1—2. 8vo. *London.* The Institution.

Institution of Mining and Metallurgy. Transactions. Vol. I. 8vo. *London* 1892–93; Bye-Laws and List of Members. 8vo. *London* 1894. The Institution.

Institution of Naval Architects. Transactions. Vol. XXXV. 4to. *London* 1894. The Institution.

Iron and Steel Institute. Journal. Vol. XLV. 8vo. *London* 1894. The Institute.

Linnean Society. Transactions (Botany). Vol. III. Parts 9—11. Vol. IV. Part 1. 4to. *London* 1893–94; Transactions (Zoology). Vol. V. Part 11. Vol. VI. Parts 1—2. 4to. *London* 1894; Journal (Botany). Vol. XXVI. No. 177. Vol. XXX. No. 209. 8vo. *London* 1894; Journal (Zoology). Vol. XXV. No. 158. 8vo. *London* 1894; Proceedings. November 1892–June 1893. 8vo. *London* 1894; List. 1893–94. 8vo. *London.* The Society.

London Mathematical Society. Proceedings. Vol. XXV. Nos. 481—494. 8vo. *London* [1894]. The Society.

Marine Biological Association. Journal. Vol. III. No. 3. 8vo. *London* 1894. The Association.

Mineralogical Society. Mineralogical Magazine. Vol. X. No. 48. 8vo. *London* 1894. The Society.

Odontological Society of Great Britain. Transactions. Vol. XXVI. No. 8. 8vo. *London* 1894. The Society.

Photographic Society. Journal and Transactions. Vol. XVIII. Nos. 10—11. 8vo. *London* 1894. The Society.

Physical Society. Proceedings. Vol. XII. Part 4. Vol. XIII. Part 1. 8vo. *London* 1894. The Society.

Royal Agricultural Society. Journal. Vol. V. Parts 2—3. 8vo. *London* 1894. The Society.

Royal College of Surgeons. Calendar. 1894. 8vo. *London.* The College.

Royal Horticultural Society. Journal. Vol. XVII. Parts 1—2. 8vo. *London* 1894. The Society.

Royal Meteorological Society. Quarterly Journal. Vol. XX. Nos. 90—91. 8vo. *London* 1894; The Meteorological Record. Vol. XIII. Nos. 51—52. 8vo. *London* [1894]; List of Fellows. 1894. 8vo. *London.* The Society.

Transactions (*continued*).

Royal Microscopical Society. Journal. 1894. Parts 3—5. 8vo. *London.* The Society.

Royal Photographic Society. Journal and Transactions. Vol. XVIII. No. 12. Vol. XIX. Nos. 1—2. 8vo. *London* 1894. The Society.

Royal Statistical Society. Journal. Vol. LVII. Parts 2—3. 8vo. *London* 1894. The Society.

Royal United Service Institution. Journal. Vol. XXXVIII. Nos. 196—200. 8vo. *London* 1894. The Institution.

St. Bartholomew's Hospital. Statistical Tables. 1893. 8vo. *London* 1894. The Hospital.

Sanitary Institute. Journal. Vol. XV. Part 2. 8vo. *London* 1894. The Institute.

Society of Antiquaries. Archæologia. Vol. LIV. Part 1. 4to. *London* 1894; Proceedings. Vol. XV. No. 1. 8vo. [*London* 1894]. The Society.

Society of Biblical Archæology. Proceedings. Vol. XVI. Parts 7—8. 8vo. *London* 1894. The Society.

Victoria Institute. Journal. Vol. XXVII. Nos. 106—108. 8vo. *London* [1894]. The Institute.

Zoological Society. Transactions. Vol. XIII. Part 9. 4to. *London* 1894; Proceedings. 1894. Parts 2—3. 8vo. *London.* The Society.

Manchester:—Geological Society. Transactions. Vol. XXII. Parts 19—21. 8vo. *Manchester* 1894. The Society.

Literary and Philosophical Society. Memoirs and Proceedings. Vol. VIII. No. 3. 8vo. *Manchester* [1894]. The Society.

Mexico:—Asociacion de Ingenieros y Arquitectos. Anales. Tomo III. Entr. 10—15. 8vo. *México* 1894. The Association.

Sociedad Científica "Antonio Alzate." Memorias y Revista. Tomo VII. Nos. 11—12. 8vo. *México* 1894. The Society.

Montreal:—McGill University. Annual Calendar. Session 1894–95. 8vo. *Montreal;* Annual Report of the Governors, Principal and Fellows. 1893. 8vo. [*Montreal* 1894.] The University.

Moscow:—Société Impériale des Naturalistes. Bulletin. 1894. No. 1. 8vo. *Moscou* 1894. The Society.

Munich:—K. B. Akademie der Wissenschaften. Sitzungsberichte (Math.-Phys. Classe). 1894. Heft 2—3. 8vo. *München.* The Academy.

Münster:—Königliche Akademie. Inaugural-Dissertationen. 1893–94. 8vo. *Münster.* The Academy.

Transactions (*continued*).

Naples:—R. Accademia delle Scienze Fisiche e Matematiche. Atti. Vol. VI. 4to. *Napoli* 1894; Rendiconto. Anno XXXIII. Fasc. 3—7. 4to. *Napoli* 1894. The Academy.

R. Accademia di Scienze Morali e Politiche. Atti. Vol. XXVI. 8vo. *Napoli* 1894; Rendiconto. Anni XXXI—XXXII. 8vo. *Napoli* 1892–93. The Academy.

Zoologische Station. Mittheilungen. Bd. XI. Heft 3. 8vo. *Berlin* 1894. The Station.

Netherlands:—Nederlandsche Botanische Vereeniging. Nederlandsch Kruidkundig Archief. Deel VI. Stuk 3. 8vo. *Nijmegen* 1894. Foreign Office, The Hague.

New York:—Academy of Sciences. Annals. Vol. V. Nos. 9—12. Vol. VI. Nos. 7—12. Vol. VII. Nos. 6—12. Vol. VIII. No. 4. 8vo. *New York* 1890–94. The Academy.

American Geographical Society. Bulletin. Vol. XXVI. Nos. 2—3. 8vo. *New York* 1894. The Society.

American Museum of Natural History. Annual Report. 1893. 8vo. *New York* 1894; Bulletin. Vol. VI. Pages 193—304. 8vo. [*New York*] 1894. The Museum.

Linnæan Society. Abstracts of the Proceedings. 1893–94. No. 6. 8vo. *New York.* The Society.

Scientific Alliance. Fourth Annual Directory. 8vo. *New York* 1894. The Alliance.

Nottingham:—University College. Calendar. 1894–95. 8vo. *Nottingham.* The College.

Odessa:—New Russian Society of Naturalists. Zapiski. Vol. XVIII. Part 2. [*Russian.*] 8vo. *Odessa* 1894. The Society.

Palermo:—Circolo Matematico. Rendiconti. Tomo VIII. Fasc. 4—5. 8vo. *Palermo* 1894. The Circle.

Paris:—Association Française pour l'Avancement des Sciences. Compte Rendu. Sessions 21—22. 8vo. *Paris* 1892–94. The Association.

Bureau Central Météorologique. Annales. 1892. 3 vols. 4to. *Paris* 1894. The Bureau.

École Normale Supérieure. Annales Scientifiques. Tome XI. Nos. 7—10. 4to. *Paris* 1894. The School.

Société de Géographie. Bulletin. Tome XV. Trim. 1. 8vo. *Paris* 1894. The Society.

Société Entomologique de France. Annales. Vol. LXIII. Trim. 1. 8vo. *Paris* 1894. The Society.

Société Française de Physique. Séances. 1893. Juillet—Décembre. 1894. Fasc. 1—2. 8vo. *Paris.* The Society.

Société Mathématique. Bulletin. Tome XXII. Nos. 5—8. 8vo. *Paris* [1894]. The Society.

Transactions (*continued*).

Société Philomathique. Bulletin. Tome VI. No. 2. 8vo. *Paris* 1894. The Society.

Philadelphia:—Academy of Natural Sciences. Journal. Vol. X. Part 1. 4to. *Philadelphia* 1894; Proceedings. 1893. Part 3. 1894. Part 1. 8vo. *Philadelphia.* The Academy.

American Philosophical Society. Proceedings. Vol. XXXIII. No. 144. 8vo. *Philadelphia* 1894. The Society.

Pisa:—Società Toscana di Scienze Naturali. Processi Verbali. Vol. IX. Pages 63—132. 8vo. [*Pisa*] 1894. The Society.

Prague:—Gesellschaft zur Förderung Deutscher Wissenschaft, Kunst und Literatur in Böhmen. Übersicht über die Leistungen der Deutschen Böhmens auf dem Gebiete der Wissenschaft, Kunst und Literatur im Jahre 1892. 8vo. *Prag* 1894. The Society.

K. Böhm. Gesellschaft der Wissenschaften. Jahresbericht. 1893. 8vo. *Prag* 1894; Sitzungsberichte (Math.-naturw. Classe). 1893. 8vo. *Prag* 1894. The Society.

Rome:—Accademia Pontificia de' Nuovi Lincei. Atti. Anno XLVI. Sessioni 4—8. Anno XLVII. Sessioni 1—3. 4to. *Roma* 1893-94. The Academy.

R. Comitato Geologico d'Italia. Bollettino. 1894. Trim. 2. 8vo. *Roma* 1894. The Committee.

Sacramento:—California State Mining Bureau. Bulletin. Nos. 3—4. 8vo. *Sacramento* 1894. The Bureau.

Saint John:—Natural History Society of New Brunswick. Bulletin. Nos. 2, 4—10. 8vo. *Saint John, N.B.* 1883-92. The Society.

St. Petersburg:—Académie Impériale des Sciences. Bulletin. Vol. XXXVI. Nos. 1—2. 8vo. *St. Pétersbourg* 1894. The Academy.

San Francisco:—Geographical Society of California. Bulletin. May, 1894. 8vo. *San Francisco.* The Society.

Santiago:—Sociedad Nacional de Mineria. Boletin. Año XI. Nos. 66—69. 4to. *Santiago de Chile* 1894. The Society.

Société Scientifique de Chili. Actes. Tome IV. Livr. 1. 8vo. *Santiago* 1894. The Society.

Siena:—R. Accademia dei Fisiocritici. Atti. Vol. VI. Fasc. 8—9. 8vo. *Siena* 1894; Processi Verbali. No. 5. 8vo. *Siena* 1894. The Academy.

Somerville, Mass.:—Tufts College. Studies. Nos. 2—3. 8vo. *Tufts College* 1894. The College.

Stockholm:—K. Svenska Vetenskaps Akademie. Handlingar. 1892. Häft 1. 4to. *Stockholm;* Öfversigt. Årg. LI.

Transactions (*continued*).
Nos. 5—7. 8vo. *Stockholm* 1894; Bihang. Bd. XIX. 8vo. *Stockholm* 1894; Lefnadsteckningar. Bd. III. Häft 2. 8vo. *Stockholm* 1894. The Academy.

Sydney:—Linnean Society of New South Wales. Proceedings. Vol. VIII. Parts 2—4. Vol. IX. Part 1. 8vo. *Sydney* 1893-94. The Society.

Royal Society of New South Wales. Journal and Proceedings. Vol. XXVII. 8vo. *Sydney* [1894]. The Society.

University. Calendar. 1894. 8vo. *Sydney.* The University.

Tōkyō:—Imperial University. Journal of the College of Science. Vol. VI. Part 4. Vol. VII. Part 1. 8vo. *Tōkyō* 1894. The University.

Toulouse:—Faculté des Sciences. Annales. Tome VIII. Fasc. 2—3. 4to. *Paris* 1894. The Faculty.

Turin:—R. Accademia delle Scienze. Atti. Vol. XXIX. Disp. 11—15. 8vo. *Torino* 1894. The Academy.

Upsala:—Kongl. Vetenskaps Societet. Nova Acta. Vol. XVI. 4to. *Upsaliæ* 1893. The Society.

Universitet. Årsskrift. 1893. 8vo. *Upsala;* Theses. 1893–94. 8vo. The University

Utrecht:—Nederlandsch Gasthuis voor Ooglijders. Oogheelkundige Verslagen en Bijbladen. No. 35. 8vo. *Utrecht* 1894. The Hospital.

Provinciaal Utrechtsch Genootschap. Aanteekeningen. 1893. 8vo. *Utrecht;* Verslag. 1893. 8vo. *Utrecht;* De Nederlandsche Ambassade-Kapel te Parijs: Kerkhistorische Studie door L. A. van Langeraad. 2 vols. 8vo. *'sGravenhage* 1893–94. The Society.

Vienna:—Anthropologische Gesellschaft. Mittheilungen. Bd. XXIV. Heft 3—4. 4to. *Wien* 1894. The Society.

Kais. Akademie der Wissenschaften. Sitzungsberichte (Math.-Phys. Classe). Bd. CIII. Abth. 1. Heft 1—7. Abth. 2a. Heft 3—7. Abth. 2b. Heft 4—5. Abth. 3. Heft 1—7. 8vo. *Wien* 1894. The Academy.

K.K. Geologische Reichsanstalt. Jahrbuch. Jahrg. 1891. Heft 4. 1894. Heft 1. 8vo. *Wien* 1894; Verhandlungen. 1894. No. 5. 8vo. *Wien.* The Institute.

K.K. Zoologisch-botanische Gesellschaft. Verhandlungen. Bd. XLIV. Quartal 1—2. 8vo. *Wien* 1894. The Society.

Washington:—Bureau of Ethnology. Tenth Annual Report. 4to. *Washington* 1893; Bibliography of the Wakashan Languages. 8vo. *Washington* 1894; The Maya Year. 8vo. *Washington* 1894; The Pamunkey Indians of Virginia. 8vo. *Washington* 1894. The Bureau.

Transactions (*continued*).

Smithsonian Institution. Annual Report. 1892. 8vo. *Washington* 1893. The Institution.

U.S. Bureau of Education. Catalog of "A. L. A." Library, selected by the American Library Association and shown at the World's Columbian Exposition. 8vo. *Washington* 1893; Statistics of Public Libraries in the United States and Canada. 8vo. *Washington* 1893. The Bureau.

U.S. National Museum. Bulletin. No. 43. 8vo. *Washington* 1893. The Museum.

Wellington:—New Zealand Institute. Transactions and Proceedings. Vol. XXVI. 8vo. *Wellington* 1894. The Institute.

Polynesian Society. Journal. Vol. III. No. 2. 8vo. *Wellington, N.Z.* 1894. The Society.

Würzburg:—Physikalisch-medicinische Gesellschaft. Sitzungs-Berichte. Jahrg. 1894. Nos. 1—4. 8vo. *Würzburg;* Verhandlungen. Bd. XXVIII. No. 1. 8vo. *Würzburg* 1894. The Society.

Zurich:—Naturforschende Gesellschaft. Vierteljahrsschrift. Jahrg. XXXIX. Heft. 2. 8vo. *Zürich* 1894. The Society.

Observations and Reports.

Alsace-Lorraine:—Meteorologisches Landesdienst. Ergebnisse der Meteorologischen Beobachtungen im Reichsland Elsass-Lothringen. 1892. 4to. *Strassburg i. E.* 1894. Meteorological Office, London.

Austria:—K.K. Gradmessungs Bureau. Astronomische Arbeiten. Bd. V. Längenbestimmungen. 4to. *Wien* 1893. The Bureau.

Berlin:—Königl. Preussisches Geodätisches Institut. Polhöhenbestimmungen im Harzgebiet ausgeführt in den Jahren 1887 bis 1891. 4to. *Berlin* 1894. The Institute.

Brisbane:—Registrar-General's Office. Statistics of the Colony of Queensland. 1893. Folio. *Brisbane* 1894; Vital Statistics. 1893. Folio. *Brisbane* 1894. The Registrar-General.

Bucharest:—Institutul Meteorologic. Analele. Tom. VIII. 4to. *Bucuresti* 1894. The Institute.

Calcutta:—Meteorological Department, Government of India. Meteorological Observations recorded at Seven Stations in India. January—May, 1894. 4to. *Calcutta;* Monthly Weather Review. January—May, 1894. 4to. *Calcutta;* India Weather Review: Annual Summary. 1893. 4to. *Calcutta* 1894; Rainfall of India. 1893. Folio. *Calcutta* 1894. The Department.

Observations and Reports (*continued*).

Survey of India Department. General Report of the Operations. 1892–93. Folio. *Calcutta* 1894. The Department.

Cape of Good Hope:—Royal Observatory. Heliometer Observations for determination of Stellar Parallax, made by D. Gill. 8vo. *London* 1893; Report of H.M. Astronomer for the Period 1879, May 26 to 1889, July 21. 4to. [1893]. The Observatory.

Chemnitz:—Königl. Sächsisches Meteorologisches Institut. Jahrbuch. Jahrg. XI. Abth. 1—3. 4to. *Chemnitz* 1894. The Institute.

Cordova:—Observatorio Nacional. Resultados. Vol. XVII. 4to. *Buenos Aires* 1894; Atlas para acompãnar á los Tomos XVI y XVII. Oblong. [*Buenos Aires*] 1893. The Observatory.

France:—Ministère des Travaux Publics. Études des Gîtes Minéraux de la France: Les Terrains Tertiares de la Bresse. 2 parts. 4to. *Paris* 1893. The Ministry.

Helsingfors:—Institut Météorologique Central. Observations Météorologiques, 1881–88. 4 vols. Folio. *Kuopio* 1893–94; Observations Météorologiques faites à Helsingfors, 1887–89, 1892. 2 vols. Folio. *Helsingfors* 1893. The Institute.

Hongkong:—Observatory. Observations and Researches. 1893. Folio. *Hongkong* 1894. The Observatory.

India:—Geological Survey. Records. Vol. XXVII. Parts 2—3. 8vo. *Calcutta* 1894. The Survey.

Great Trigonometrical Survey. Synopsis of the Results of Operations. Vol. XXXIII. 4to. *Dehra Dun* 1893; Spirit-levelled Heights: No. 6, Bombay Presidency, Season 1890-91. 8vo. *Dehra Dun* 1892. The Survey.

Revenue and Agricultural Department, Government of India. Memorandum on the Snowfall in the Mountain Districts bordering Northern India and the Abnormal Features of the Weather in India during the past five months. Folio. *Simla* 1894. Meteorological Office, Calcutta.

International:—Association Géodésique Internationale. Comptes-Rendus des Séances de la Commission Permanente. 1893. 4to. *Neuchatel* 1894. The Central Bureau.

London:—Admiralty. Deep Sea Exploration in Faeroe Channel, by H.M.S. "Triton," 1882, commanded by Staff-Commander T. H. Tizard. Folio. [*London* 1894]; Longitude of Malacca, determined by means of Telegraphic Signals exchanged with Singapore, 1893. Folio. *London* 1894; Report on the Results of Dredgings obtained on the Macclesfield Bank, China Sea, by P. W. Bassett-Smith. Folio. *London* 1894. The Hydrographer.

Observations and Reports (*continued*).

Local Government Board. Report of the Medical Officer. 1892-93. 8vo. *London* 1894. The Medical Officer.

Lyme Regis:—Rousdon Observatory. Meteorological Observations. 1893. 4to. *London* 1894. Mr. C. E. Peek.

Madras:—Government Observatory. Results of Observations of the Fixed Stars made with the Meridian Circle. Vol. VII. 4to. *Madras* 1894. The Observatory.

Mauritius:—Royal Alfred Observatory. Results of Meteorological Observations. 1893. Folio. [*Mauritius* 1894.] The Observatory.

Melbourne:—Department of Mines. Annual Report. 1893. Folio. *Melbourne* 1894. The Department.

Observatory. Record of Results of Observations in Meteorology and Terrestrial Magnetism. October, 1893 to March, 1894. 8vo. *Melbourne* 1894. The Observatory.

Mount Hamilton:—Lick Observatory. Publications. Vol. II. 4to. *Sacramento* 1894. The Observatory.

New South Wales:—Geological Survey. Memoirs: Geology. No. 5. 4to. *Sydney* 1894; Records. Vol. IV. Part 1. 4to. *Sydney* 1894. The Survey.

Niagara:—State Reservation. Tenth Annual Report of the Commissioners. 8vo. *Albany* 1894. The Commissioners.

Oxford:—Radcliffe Observatory. Catalogue of 6,424 Stars for the Epoch 1890. 4to. *Oxford* 1894. The Observatory.

Prague:—K. K. Sternwarte. Magnetische und Meteorologische Beobachtungen. Jahrg. LIV. 4to. *Prag* 1894. The Observatory.

Salonica:—Station Météorologique. [Observations. 1893-94.] 8vo. *Salonique* 1894. The Station.

Sydney:—Department of Mines and Agriculture. Annual Report. 1893. Folio. *Sydney* 1894. The Department.

Observatory. Meteorological Observations. January—May, 1894. 8vo. [*Sydney*.] The Observatory.

United Kingdom:—Geological Survey. Memoirs: The Jurassic Rocks of Britain. Vol. IV. 8vo. *London* 1894. The Survey.

Washington:—U. S. Department of Agriculture. Experiment Station Record. Vol. V. Nos. 8—9. 8vo. *Washington* 1894. The Department.

U.S. Fish Commission. Bulletin. Vol. XI. 8vo. *Washington* 1893. The Commission.

U.S. Geological Survey. Monographs. Vols. XIX, XXI—XXII. 4to. *Washington* 1892-93; Mineral Resources of the United States. 1892-93. 8vo. *Washington* 1893-94; Bulletin. Nos. 97—117. 8vo. *Washington* 1893-94. The Survey.

Observations and Reports (*continued*).

Weather Bureau. Maryland State Weather Service. Vol. III. Nos. 1—2, 4, 6—8, 11—12. 4to. *Baltimore* 1893–94; The Climatology and Physical Features of Maryland. 8vo. *Baltimore* 1894; Monthly Weather Review. April—July, 1894. 4to. *Washington* 1894; Annual Summary. 1893. 4to. *Washington* 1894; Report of the Chief of the Weather Bureau. 1893. 8vo. *Washington* 1894; Report of the Ohio Weather and Crop Service. June—August, 1894. 8vo. *Norwalk;* Protection from Lightning: by A. McAdie. 8vo. *Washington* 1894; Weather Bureau. Report of the International Meteorological Congress held at Chicago, August, 1893. Part 1. 8vo. *Washington* 1894. The Bureau.

Windsor, N.S.W.—Mr. Tebbutt's Observatory. Report. 1893. 8vo. *Sydney* 1894. Mr. J. Tebbutt.

Journals.

Agricultural Gazette of New South Wales. Vol. V. Parts 4—8. 8vo. *Sydney* 1894. Department of Agriculture, Sydney.

American Journal of Philology. Vol. XV. No. 2. 8vo. *Baltimore* 1894. The Editor.

Annaes de Sciencias Naturaes. Anno I. No. 3. 8vo. *Porto* 1894. The Editor.

Archives des Sciences Biologiques. Tome III. No. 1. 8vo. *St. Pétersbourg* 1894. Institut Impérial de Médecine Expérimentale, St. Petersburg.

Asclepiad. Vol. XI. No. 41. 8vo. *London* 1894. Sir B. W. Richardson, F.R.S.

Astronomische Nachrichten. Bd. CXXXV. 4to. *Kiel* 1894. Königl. Sternwarte, Kiel.

Astronomy and Astro-Physics. August, 1894. 8vo. *Northfield, Minn.* The Editors.

Boletin de Minas Industria y Construcciones. Año X. Num. 5—6. 4to. *Lima* 1894. Escuela Especial de Ingenieros.

Cellule (La). Tome X. Fasc. 2. Roy. 8vo. *Lierre* 1894. The Editors.

Epigraphia Indica. Vol. II. Part 15. Vol. III. Parts 1—4. 4to. *Calcutta* 1894. Superintendent of Government Printing.

Horological Journal. Vol. XXXVI. No. 431—435. *London.* British Horological Institute.

Journal of Comparative Neurology. Vol. IV. Pp. 73—192. 8vo. *Granville, Ohio* 1894. The Editors.

Journals (*continued*).

Mathematical Magazine. Vol. II. No. 8. 4to. *Washington* 1894.
The Editor.

Medico-Legal Journal. Vol. XI. No. 4. 8vo. *New York* 1894.
The Editor.

Morphologisches Jahrbuch. Bd. XXI. Heft 3. 8vo. *Leipzig* 1894.
Professor C. Gegenbaur, For. Mem. R.S.

Nature Notes. Vol. V. Nos. 55—59. *London* 1894.
Selborne Society.

Physical Review. Vol. II. No. 1. 8vo. *New York* 1894.
The Editors.

Sbornik Materialov dlya Opisaniya Myestenosteii i Plemen Kavkaza. Vol. XVIII. 8vo. *Tiflis* 1894.
Curateur de l'Arrondissement Scolaire du Caucase.

Stazioni Sperimentali Agrarie Italiane. Vol. XXV. Fasc. 5—6. Vol. XXVI. Fasc. 4—6. Vol. XXVII. Fasc. 1. 8vo. *Modena* 1893–94.
R. Stazione Agraria, Modena.

Technology Quarterly. Vol. VI. No. 4. Vol. VII. No. 1. 8vo. *Boston* 1893–94.
Massachusetts Institute of Technology.

Zeitschrift für Biologie. Bd. XXXI. Heft 2—3. 8vo. *München* 1894.
The Editors

Zeitschrift für Naturwissenschaften. Bd. LXVII. Heft 1—2. 8vo. *Leipzig* 1894.
Naturw. Verein für Sachsen u. Thüringen, Halle.

Albert 1er de Monaco (Prince) Résultats des Campagnes Scientifiques accomplies sur son Yacht. Fasc. VII. Partie 1. 4to. *Monaco* 1894.
The Prince of Monaco.

Alcock (A.) and Anderson (A. R.) Natural History Notes from H.M. Indian Marine Survey Steamer "Investigator." Series 2. Nos. 11—14. 8vo. *Calcutta* 1894.
The Authors.

Bleicher (M. G.) Contribution à l'Étude des Bryozoaires et des Spongiaires de l'Oolithe Inférieure de Meurthe-et-Moselle. 8vo. *Nancy* [1894]; Sur la Structure de certaines Rouilles; leur Analogie avec celles des Minerais de Fer Sédimentaires de Lorraine. 4to. *Paris* [1894].
The Author.

Brodie (Rev. P. B.) [Five Excerpts.] 8vo. *Warwick* [1893].
The Author.

Buller (Sir W. L.), F.R.S. On the Birds observed during a Voyage from New Zealand to England. 8vo. [*Wellington, N.Z.* 1894.]
The Author.

Cayley (A.), F.R.S. Collected Mathematical Papers. Vol. VII. 4to. *Cambridge* 1894.
The Author.

Chatterjee (M. N.) A Manual of Deductive Logic for Students . . . of the Indian Universities. 8vo. *Lahore* 1892. The Author.

Coghlan (T. A.) The Wealth and Progress of New South Wales. 1893. 8vo. *Sydney.* The Agent-General for N.S.W.

Colenso (Rev. W.), F.R.S. [Three Excerpts from Trans. N.Z. Inst.] 8vo. [*Wellington* 1894.] The Author.

Collins (F. H.) Supplement to Epitome of the Synthetic Philosophy. 8vo. *London* 1894; Twelve Charts of the Tidal Streams of the North Sea and its Coasts. 4to. *London* 1894. The Author.

Cook (James), F.R.S. Captain Cook's Journal during his First Voyage round the World, made in H.M. Bark "Endeavour," 1768–71: A Literal Transcription of the Original MSS., with Notes and Introduction. Edited by W. J. L. Wharton. 8vo. *London* 1893. Captain Wharton, R.N., F.R.S.

Davidson (G.) Variation of Latitude at San Francisco, 1891–92. 4to. *Lynn, Mass.* [1894]. The Author.

Dawson (W. B.) Survey of Tides and Currents in Canadian Waters. 8vo. [*Ottawa* 1894.] The Author.

De Heen (P.) Détermination du Volume des Liquides et des Gaz au-dessus de la Température Critique. 8vo. *Bruxelles* [1894]. [And four other Excerpts.] 8vo. The Author.

Delsol (E.) Note sur la Théorie des Parallèles. 12mo. *Santa-Fé* 1894. The Author.

Dyer (B.) Fertilisers and Feeding Stuffs. 8vo. *London* 1894. The Author.

Font y Torné (M.) De la Filariosis. 8vo. *Barcelona* 1894. The Author.

Hale (H.) Two Essays in Folk-lore: The Fall of Hochelaga, a Study of Popular Tradition; and "Above" and "Below," a Mythological Disease of Language. 8vo. *Cambridge, Mass.* 1890–94. The Author.

Harlé (E.) Restes d'Elan et de Lion dans une Station Préhistorique de transition entre le Quaternaire et les Temps Actuels à Sainte-Martory (Haute Garonne). 8vo. [*Paris* 1894]. The Author.

Helmholtz (H. von), For. Mem. R.S. Handbuch der Physiologischen Optik. Lief. 8. 8vo. *Hamburg* 1894. The Author.

Hering (H. S.) and **Aldrich** (W. S.) Test of the Neversink Mountain Electric Road. 4to. *New York* 1892. The Authors.

Hincks (Rev. T.), F.R.S. Marine Polyzoa: Contributions towards a General History. 8vo. 1890–91. The Author.

Hinrichs (G.) Notes sur la Mécanique des Atomes. Séries 2—3. 4to. *Paris* 1891–92; Sechs Beiträge zur Dynamik des Chemischen Moleküls. 8vo. *Leipzig* 1892. The Author.

Horton-Smith (R.) The Theory of Conditional Sentences in Greek and Latin. 8vo. *London* 1894. The Author.

Keeler (J. E.) On the Spectra of the Orion Nebula and the Orion Stars. 8vo. [*Northfield, Minn.* 1894]; Spectroscopic Observations of Nebulæ, made at Mount Hamilton, California, with the Thirty-six Inch Refractor of the Lick Observatory. 4to. 1894. The Author.

Klossovsky (A.) Distribution Annuelle des Orages à la Surface du Globe Terrestre. 4to. *Odessa* 1894; Organisation de l'Étude Climatérique Spéciale de la Russie et Problèmes de la Météorologie Agricole. 4to. *Odessa* 1894. The Author.

Kölliker (A. von), For. Mem. R.S. Der feinere Bau und die Functionen des sympathischen Nervensystems. 8vo. *Würzburg* 1894; Ueber den Fornix longus von Forel und die Riechstrahlungen im Gehirn des Kaninchens. 8vo. [*Jena* 1894]. The Author.

Kops (J.) Flora-Batava. Aflev. 305—306. 4to. *Leiden* [1894]. The Netherlands Government.

Lemoine (É.) [4 Mathematical Excerpts.] 8vo. The Author.

Lingard (A.) Summary of Further Report on Surra. Folio. *Bombay* 1894. The Author.

Lobatchefsky (N. I.) Celebration of the One Hundred Year's Anniversary of the Birth of N. I. Lobatchefsky by the Imperial Kazan University. 1793–1893. [*Russian.*] Folio. *Kazan* 1894. The University.

Lorentzen (G.) Ueber die Untersuchung der Scalen eines Heliometers. 4to. [*Kiel* 1894.] The Author.

Lummer (Dr.) Ueber die Ziele und die Thätigkeit der Physikalisch-Technischen Reichsanstalt: Vortrag. 4to. *Berlin* 1894. The Author.

Marignac (J. C. G. de), For. Mem. R.S. Notice Biographique par E. Ador. 8vo. *Genève* 1894. The Author.

Marsh (O. C.) [3 Excerpts from the 'Amer. Journ. Sci.'] 8vo.; Description of Tertiary Artiodactyles. 8vo. [*New Haven*] 1894. The Author.

Martel (E. A.) Les Abîmes, les Eaux Souterraines, les Cavernes, les Sources, la Spélæologie. 4to. *Paris* 1894. The Author.

Mouret (G.) Démonstration du Principe de l'Équivalence. 8vo. *Niort* 1894. The Author.

Norman (J. H.) The Scientific Method of working the World's Foreign and Colonial Exchanges. 8vo. *London* 1894. The Author.

Prinz (W.) Agrandissements de Photographies Lunaires. Folio. [*Bruxelles* 1894.] The Author.

Ramsay (A.) The Scientific Roll and Magazine of Systematized Notes. Climate. Nos. 1—5. 8vo. *London* 1890–94. Mr. Ramsay.

Ranke (J.) Der Mensch. 2 vols. 8vo. *Leipzig* 1894; Beiträge zur physichen Anthropologie der Bayern. Bd. II. 4to. *München* 1892. The Author.

Roberts (I.), F.R.S. A Selection of Photographs of Stars, Star-clusters and Nebulæ. 4to. *London* [1894.] The Author.

Rodrigues (J. B.) Plantas Novas cultivadas no Jardim Botanico do Rio de Janeiro. No. 4. 4to. *Rio de Janeiro* 1894. The Author.

Russell (H. C.), F.R.S. [Address to Section A of the Australasian Association for the Advancement of Science, Brisbane, 1894.] 8vo. The Author.

Sabatier (A.) Études sur le Cœur et la Circulation Centrale dans la Série des Vertébrés. 4to. *Montpellier* 1873; De la Spermatogenèse chez les Crustacés Décapodes. 8vo. *Montpellier* 1893. The Author.

Sanders (A.) Researches in the Nervous System of *Myxine glutinosa.* 4to. *London* 1894. The Author.

Schreiber (P.) Die Zustandsgleichungen einer Luftsäule (Barometrische Höhenformel). 4to. [1894.] The Author.

Sharpe (R. R.) London and the Kingdom: a History derived mainly from the Archives at Guildhall in the Custody of the Corporation of the City of London. Vols. I—II. 8vo. *London* 1894. The Corporation.

Stourdza (Prince Grigori) Les Lois Fondamentales de l'Univers. 8vo. *Paris* 1891. The Author.

Symons (G. J.), F.R.S., and Wallis (H. S.) British Rainfall. 1893. 8vo. *London* 1894. Mr. G. J. Symons, F.R.S.

Thomson (J. J.), F.R.S. Notes on Recent Researches in Electricity and Magnetism. 8vo. *Oxford* 1893. The Author.

Vernon-Harcourt (L. F.) and Partiot (H. L.) The Training of Rivers and Estuaries. 8vo. *London* 1894. Mr. Vernon-Harcourt.

Very (F. W.) Hail-Storms. 8vo. [*Pittsburgh*] 1894. The Author.

Ward (J. J.) The Hidden Pyramid; or the Mystery of the Great Pyramid Explained. 8vo. *Leeds.* The Author.

Winlock (W. C.) Progress of Astronomy for 1891 and 1892. 8vo. *Washington* 1893. Smithsonian Institution.

Wolfer (A.) Astronomische Mitteilungen. No. 84. 8vo. [*Zürich*] 1894. The Author.

Wood-Mason (J.) and Alcock (A.) Natural History Notes from H.M. Indian Marine Survey Steamer "Investigator." Series II. No. 1. On the Results of the Deep-Sea Dredging during the Season 1890–91. 8vo. [*London*] 1891–94. Dr. Alcock.

Young (Thomas) Œuvres Ophtalmologiques: Traduites et Annotées par M. Tscherning. 8vo. *Copenhague* 1894. The Translator.

Bronze Medal, struck in honour of Dr. John Syer Bristowe, F.R.S. The obverse bears the effigy of Dr. Bristowe; the reverse depicts a student in his laboratory, and has the words "St. Thomas's Hospital" in the exergue. Mr. Allan Wyon.

November 22, 1894.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair, and the list of Officers and Council nominated for election was read as follows:—

President.—The Lord Kelvin, D.C.L., LL.D.

Treasurer.—Sir John Evans, K.C.B., D.C.L., LL.D.

Secretaries.— { Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Sir Joseph Lister, Bart., F.R.C.S.

Other Members of the Council.—Andrew Ainslie Common, LL.D.; William Crookes, F.C.S.; Francis Darwin, M.A.; Andrew Russell Forsyth, Sc.D.; Sir Douglas Galton, K.C.B.; Professor Alexander Henry Green, M.A.; Sir John Kirk, K.C.B.; Professor Horace Lamb, M.A.; Professor Edwin Ray Lankester, M.A.; Professor Alexander Macalister, M.D.; Professor John Henry Poynting, D.Sc.; Professor Arthur William Rücker, M.A.; Osbert Salvin, M.A.; Professor J. S. Burdon Sanderson, M.D.; Thomas Edward Thorpe, Sc.D.; William Henry White, C.B.

The following Papers were read:—

I. "A Determination of the Specific Heat of Water in terms of the International Electric Units." By ARTHUR SCHUSTER, F.R.S., Langworthy Professor of Physics at the Owens College, Manchester, and WILLIAM GANNON, M.A., Exhibition (1851) Scholar, Queen's College, Galway. Received November 13, 1894.

(Abstract.)

This research was originally undertaken by Professor Schuster and Mr. H. Hadley, before the authors were aware that Mr. E. H. Griffiths was engaged on a similar investigation. After a number of preliminary experiments, and just as the final arrangements for the conduct of the measurements were being definitely made, Mr. Hadley, on his appointment to the Head Mastership of the School of Science and Art, Kidderminster, had to leave Manchester. In the meantime Mr. Griffiths' important research was published, and we had to consider whether our own work, which was designed on a smaller scale, could compete with it in accuracy. We decided to complete the investigation, principally for the reason that, although we both aimed at determining what is commonly called the mechanical equivalent of heat through the heating of a certain mass of water by means of an electric current, the details of the experiments differed very materially, so that our two ways of dealing with the problem seemed to afford a useful test of the amount of agreement which at present may be obtained. Our investigation touches only a small part of that treated by Mr. Griffiths, as we did not attempt to measure the changes in the specific heat of water due to change of temperature. On the other hand, the more modest limits within which we have confined ourselves allowed us to use a much simpler apparatus.

On Mr. Hadley's departure, Mr. W. Gannon took his place. From the former gentleman we received a good deal of help in the devising and construction of some important parts of the apparatus.

The principle of the method we have used is extremely simple. The electrical work done in a conductor being measured by $\int ECdt$, where E is the difference of potential at the ends of the conductor, C the current, and t the time. We keep the electromotive force constant, and measure $\int Cdt$ directly by a silver voltameter. We do not, therefore, require to know the resistance of the wire, and we thus avoid the difficulty of having to estimate the excess of temperature of the wire over that of the water in which it is placed. We also gain the advantage of not having to measure time, and therefore of being able to complete the experiments more quickly than we could

have safely done if the length of time the current passed had to be measured with great accuracy.

The Results.

We divide our experiments into three series, but only attach any value to the third, as during the first two sets the thermometer was falling in the last period. We had hoped at first to be able to apply a small correction to eliminate the error which is due to the fact that a falling thermometer will read too high, and we made a large number of experiments to determine the correction. We arrived, however, at the result that the indications of a falling thermometer are so irregular that no correction is possible. Consequently in our third series the thermometer was rising during the whole course of each experiment.

We think it worth while to put on record an impression that the behaviour of our Baudin thermometer has altered since we first received it from the maker. As soon as it came some preliminary experiments were made to see if we could work with the thermometer while it was falling, and the observations seemed to show that the fall was sufficiently uniform. Our first three experiments gave results which were very consistent, and a minute after the current was broken the temperature seemed to fall already in a perfectly regular manner. But as we continued our work, the behaviour of the instrument seemed to deteriorate. Thus, in our experiment on March 8, the temperature had hardly fallen three minutes after the current had stopped. It is possible that this is due to accidental circumstances, for it is well known that different places of the bore of a capillary tube behave very differently. We wish, therefore, to express no opinion at present as to the probability of an actual change in the thermometer, but only to draw the attention of other experimenters to this point, which seems worth keeping in mind.

Our final value is

J = 4·1804 Joules on the mercury scale of hard French glass,
4·1905 on the nitrogen scale,
4·1917 on the hydrogen scale,
at a temperature of 19°·1.

This result depends on the assumption that the electrochemical equivalent of silver is 0·001118, and that our standard Clark cell had an electromotive force of

$$1{\cdot}4340(1-\alpha\,\overline{t^{\circ}-15})\text{ volts},$$

where

$$\alpha = 0{\cdot}000814+0{\cdot}000007(t^{\circ}-15),$$

as given by Kahle ('Zeits. f. Instrumentenkunde,' vol. 13, p. 310, 1893). Glazebrook and Skinner's coefficient refers to a mean temperature of 7°·5, and is identical with the above at that temperature.

The comparison of the results of different observers will be facilitated by Table I, in which we compare the work done in ergs with the foot-pound at Greenwich and the kilogrammetre at Paris. This table has been calculated on the assumption that g at Paris is equal to 980·96, and at Greenwich equal to 981·24. We have prepared another table (II), which at any temperature will give the correction of an interval measured on our mercury thermometer to an interval measured on the nitrogen and hydrogen thermometers. This table has been calculated with the help of the equation given by Chappuis for the correction to the thermometer made of French hard glass.

In comparing our results with that of other observers, we have in the first place to consider the value which Mr. Griffiths has obtained in his very excellent series of measurements. His final result ('Roy. Soc. Proc.,' vol. 55, p. 26; 'Phil. Trans.,' clxxxiv, A, 1893) is

$$J = 4{\cdot}1982(1-0{\cdot}00266\,\overline{\theta-15})\times 10^7.$$

This refers to the nitrogen thermometer. At a temperature of 19·1°, the value would be reduced to 4·1936, which corresponds to our 4·1905 at the same temperature. Griffiths' value is to be increased slightly, owing to the fact that he really measures the difference between the specific heat of water and of air. This would increase the value of J by ·0011 about, so that the value of J at 19·1° would be raised to $4{\cdot}1947 \times 10^7$, which is exactly one part in a thousand larger than ours. The difference is small, but must be due to some systematic error, as both Griffiths' value and our own agree so well with each other, that ordinary observational errors and accidental disturbances could not have produced so large a difference in our results. The least satisfactory part of a calorimetric measurement must always be the application of the cooling correction, and we have considered it of great importance to reduce that correction as much as possible. The uncertainty of the cooling correction does not necessarily depend on its value; thus we can much diminish it by starting, as we have done in the third series, with the initial temperature of the calorimeter about as much below that of the water jacket as the final temperature is above it; yet the *uncertainty* of the correction does not seem to us to be diminished by that process. We may reasonably estimate the uncertainty due to the cooling correction, by calculating what the error in the observed rate of cooling, either at the beginning or the end of the experiment, must have been in order to produce a difference of one part in a thousand in the final result. We find in our own experiments that the error must

Table I.

Ergs × 10^{-7}.	Foot-pounds at Greenwich.	Kilogrammetres at Paris.
4·160	772·83	424·07
4·165	773·76	424·58
4·170	774·68	425·09
4·175	775·61	425·60
4·180	776·54	426·11
4·185	777·47	426·62
4·190	778·40	427·13
4·195	779·33	427·64
4·200	780·25	428·15
4·205	781·19	428·66
4·210	782·12	429·17

Table II.

	10°.	12°.	14°.	16°.	18°.	20°.	22°.	24°.
Correction to nitrogen thermometer	−0°·00369	0°·00337	0°·00305	0°·00275	0°·00247	0°·00220	0°·00194	0°·00170
Correction to hydrogen thermometer	−0°·00418	0°·00381	0°·00346	0°·00313	0°·00280	0°·00249	0°·00219	0°·00190

have amounted to more than 15 per cent. We consider it unlikely that so large an error occurred always in the same direction. Apart from the cooling correction, however, it is difficult to see how a difference of one-tenth per cent. in our result can be produced unless by the accumulation of a number of small errors.

The weak point of Mr. Griffiths' determination is the small quantity of water he uses, his result depending on the difference in his rate of rise after the addition of about 120 grams into his calorimeter. The highest water equivalent with which he worked was about 360 grams, of which 85 are due to the calorimeter itself. The chief objection to the use of such small quantities of water lies in the great cooling correction. In the experiments quoted by him at p. 482, the loss of heat due to radiation and conduction at the end of his experiment amounts to about 9 per cent. of his heat supply. If such loss had been wrongly estimated to 1 per cent., an error of one-tenth per cent. would result in the final value. The consistency of Mr. Griffiths' results shows that if an error occurred due to that cause, it must have been systematic; and we may point out how, with such a large cooling correction, serious errors may arise. In applying the cooling correction, it is always assumed that the loss of heat depends only on the difference of temperature between the calorimeter and enclosure; but, as has been already pointed out, this is not the case as regards conduction. Mr. Griffiths' calorimeter was suspended by three stout glass tubes, through one of which the stirrer was passing. The exhaustion in the space surrounding the calorimeter was never sufficient to do away with the conduction of air; so that we may take the larger part of the cooling to be due to conduction and convection. The loss of heat in that case must to some extent depend, not only on the temperature, but also on the *rate of change* of temperature. Whether the part which depends on the rate of change is sufficient to produce a sensible difference in the result, it is not easy to say. But the error produced would, with different currents and quantities of water, be the same in all cases, and could not therefore be detected by the inconsistencies thereby introduced into the results.

The difference between our value of the equivalent and that of Mr. Griffiths are, however, of smaller importance than the difference which exists between them and the equivalent as determined directly by Joule, Rowland, and Miculescu. Joule's latest value, which is the only one which needs consideration, is 772·65 foot-pounds, at 61°·7 Fah. The number refers to the degree as measured by Joule's mercury thermometer. Rowland adds to this a correction to the air thermometer of about 3, and another small correction for a change in the heat capacity of the apparatus, which brings the value up to about 776. The correction to the air thermometer has been obtained by means of a comparison made by Joule himself with one of Rowland's thermometers. Joule's original thermometers have been temporarily placed by Mr. B. A. Joule in the hands of Professor Schuster, in order that an accurate comparison may be instituted between it and modern thermometers. A full description of the comparisons made will be given on another occasion. The result arrived at shows that the correction is less than that assumed by Rowland, and would bring his value up only to 775 at the temperature indicated. Griffiths compares his result with that deduced by Rowland from Joule's observations. Rowland combined the different values obtained by Joule in his various investigations, attaching weights according to his judgment as to their relative merits. He finds in this way that the difference between him and Joule amounts to one part in 350, but if equal weights are attached to all Joule's results, the difference is reduced to one part in 4281. Little value can be attached, however, to a combination of Joule's results which gives equal weights to that obtained in 1847 and that deduced from his latest and most careful work. There is, moreover, in Rowland's table a misprint or error in the reduction of Joule's 1847 result from foot-pounds to kilogram-metres, which lowers the value as given by Griffiths from 779·2 to about 778. It does not seem to us advisable to go beyond Joule's 1878 results, and the value assigned by him in this latest research should be taken as giving his final judgment on the matter. Reducing to the nitrogen thermometer of the Bureau International, Joule's result is 775 foot-pounds at Greenwich, at a temperature of 16°·5 C. At the same temperature Griffiths' number is 779·8.

Great weight must be attached to Rowland's determination, which at the temperature to which Joule's number applies is 777·6, and at 19°·1, 776·1, corresponding to our 778·5. Rowland's value is therefore halfway between our and Joule's result. But it must be taken into consideration that if the comparison between Rowland's and Joule's thermometers as made by the latter is to be trusted, Rowland's value referred to the "Paris" nitrogen thermometer would be slightly reduced. At any rate it seems probable that if his value is in error, it is rather in the direction of being too high. We have therefore a difference of three parts in a thousand to account for between our result and that of Rowland, and of nearly four parts in a thousand between Griffiths and Rowland at a temperature of 19°·1. These results are summarised in the following table :—

Table III.—Equivalent in foot-pounds at Greenwich at 19°·1 referred to the "Paris" Nitrogen Thermometer.

Joule.	Rowland.	Griffiths.	Schuster and Gannon.
774	776·1	779·1	778·5

We now turn to an investigation of Miculescu ('Annales de Chimie et de Physique,' vol. 27, 1892), in which the mechanical equivalent of heat is measured directly by what seems a very excellently devised series of experiments. Its result is $4·1857 \times 10^7$. He does not state the exact temperature to which this applies, but all his experiments seem to have been made between 10° and 13°, so that we may assume $11°·5$ to be the mean temperature of his experiments. Rowland's value at that temperature is $4·1999 \times 10^7$. We must draw attention to one point in Miculescu's work which requires clearing up before we can give to it any decisive value. Everything in the experiments depends on the measurement of a couple, the arm of the couple being the distance between two knife-edges; one of them had to support a weight of more than 43 kilograms. The distance between these knife-edges is said to have been 28 cms. in all experiments. Very insufficient information is given, however, as to how that distance was measured, and it would almost seem as if the author had trusted to the maker in adjusting the central knife-edge to the zero point of that scale. If the apparatus is still in existence, it might be well to make sure that no error has been introduced through a wrong estimate of the distance of the lever arm.

In order to compare Miculescu's value with that of others, we must apply a temperature correction which is somewhat doubtful; but taking the mean of Rowland's and Griffiths' values as the most probable at present, we obtain at 15° the following table:—

Table IV.—Equivalent in foot-pounds at Greenwich at 15° referred to the "Paris" Nitrogen Thermometer.

Joule.	Rowland.	Miculescu.	Griffiths.	Schuster and Gannon.
775	778·3	776·6	780·2	779·7

If we remember that Rowland's number referred to the "Paris" nitrogen thermometer would probably be smaller by one unit, we are struck with the fair agreement there is, on the one hand, between the results of Joule, Rowland, and Miculescu, and on the other hand between Griffiths and ourselves.

As far as we can draw any conclusions from the comparison, it seems to point to a difference in the value obtained by the electrical and direct methods. Whether this difference is due to some remaining error in the electrical units, or to some undiscovered flaw in the method adopted by Mr. Griffiths and ourselves, remains to be decided by further investigation.

II. "On the Temperature of the Carbons of the Electric Arc; with a Note on the Temperature of the Sun." By W. E. WILSON and P. L. GRAY. Communicated by Dr. G. J. STONEY, F.R.S. Received November 14, 1894.

[Publication deferred.]

III. "Observations of Sun-spot Spectra, 1879—1894." By J. NORMAN LOCKYER, C.B., F.R.S. Received November 15, 1894.

[Publication deferred.]

Presents, November 22, 1894.

Transactions.

Australia:—Australasian Association for the Advancement of Science. Report of the Fifth Meeting, 1893. 8vo. *Sydney.* The Association.

Buitenzorg:—'s Lands Plantentuin. Verslag. 1893. 8vo. *Batavia* 1894. The Garden.

Calcutta:—Asiatic Society of Bengal. Journal. Vol. LXIII. Part 2. No. 2. 8vo. *Calcutta* 1894; Proceedings. 1894. Nos. 7—8. 8vo. *Calcutta.* The Society.

Delft:—École Polytechnique. Annales. Tome VIII. Livr. 1—2. 4to. *Leide* 1894. The School.

Heidelberg:—Universität. Akademische Schriften. 1893–94. 8vo. The University.

La Plata:—Museo. Revista. Tomo V. 8vo. *La Plata* 1894. The Museum.

Liverpool:—University College. Annual Report of the Council. 1894. 8vo. *Liverpool.* The College.

London:—Institution of Mining and Metallurgy. Transactions. Vol. II. 8vo. *London* 1894. The Institution.

Sanitary Institute. Journal. Vol. XV. Part 3. 8vo. *London* 1894. The Institute.

University College. Calendar. 1894–95. 8vo. *London* 1894. The College.

Madras:—Government Museum. Bulletin. No. 1. 8vo. *Madras* 1894. The Museum.

Melbourne:—Industrial and Technological Museum. Descriptive Catalogue of the Specimens of Rocks of Victoria. 8vo. *Melbourne* 1894. Public Library, Melbourne.

Transactions (*continued*).

Paris:—École Normale Supérieure. Annales. Tome XI. No. 11. 8vo. *Paris* 1894. The School.

Société de Géographie. Bulletin. Tome XV. Trim. 2. 8vo. *Paris* 1894. The Society.

Rome:—R. Comitato Geologico d'Italia. Bollettino. Vol. XXV. Trim. 3. 8vo. *Roma* 1894. The Committee.

Rostock:—Universität. [Theses. 1893–94.] 8vo. The University.

Sydney:—Geological Survey of New South Wales. Records. Vol. IV. Part 2. 8vo. *Sydney* 1894. Department of Mines, Sydney.

Vienna:—Akademie der Wissenschaften. Sitzungsberichte (Math.-naturw. Classe). Bd. CIII. Abth. 2b. Heft 6—7. 8vo *Wien* 1894. The Academy.

Anthropologische Gesellschaft. Mittheilungen. Bd. XXIV. Heft 5. 4to. *Wien* 1894. The Society.

Observations and Reports.

Albany:—New York State Museum. Annual Report. 45—46. 8vo. *Albany* 1892–93; Bulletin. Vol. III. No. 11. 8vo. *Albany* 1893. The Museum.

Birkenhead:—Liverpool Observatory. Extracts from the Report of the Director of the Observatory, and Meteorological Results. 1892–93. 8vo. *Liverpool* 1894. The Director.

Bombay:—Government Observatory. Magnetical and Meteorological Observations. 1893. 4to. *Bombay* 1894. The Observatory.

Calcutta:—Meteorological Department, Government of India. Report on the Administration. 1893–94. Folio. *Calcutta.* The Department.

Edinburgh:—Museum of Science and Art. Report of the Director. 1893. 8vo. [*Edinburgh* 1894.] The Director.

Geneva:—Résumé Météorologique de l'Année 1893 pour Genève et le Grand Saint-Bernard. 8vo. *Genève* 1894. The Author.

Greenwich:—Royal Observatory. Astronomical and Magnetical and Meteorological Observations. 1891. 4to. *London* 1893; Results of Astronomical Observations. 1891. 4to. *London* 1893; Results of the Magnetical and Meteorological Observations. 1891. 4to. *London* 1893; Results of the Spectroscopic and Photographic Observations. 1891. 4to. *London* 1893; Five-Year Catalogue of 258 Fundamental Stars, deduced from Observations extending from 1887 to 1891. 4to. *London* 1893. The Observatory.

Observations and Reports (*continued*).

Madras:—Government Museum. Administrative Report. 1893–94. Folio. *Madras* 1894. The Museum.

Sydney:—Australian Museum. Report of the Trustees. 1893. Folio. *Sydney* 1894. The Trustees.

Vienna:—v. Kuffner'sche Sternwarte. Publicationen. Bd. III. 4to. *Wien* 1894. The Observatory.

Washington:—U. S. Weather Bureau. Report of the Ohio Weather and Crop Service. September, 1894. 8vo. *Norwalk, Ohio* 1894. The Bureau.

Journals.

Jornal de Sciencias Mathematicas, Physicas e Naturaes. Sér. 2. Tom. II. Num. 8. Tom. III. Num. 9—11. 8vo. *Lisboa* 1892–94. Royal Academy of Sciences, Lisbon.

Physical Review. Vol. II. No. 3. 8vo. *New York* 1894. The Editors.

Timehri. Vol. VIII. Part 1. 8vo. *Demerara* 1894. Royal Agricultural and Commercial Society of British Guiana.

Bombarda (M.) Contribuição para o Estudo dos Microcephalos. 8vo. *Lisboa* 1894. Royal Academy of Sciences, Lisbon.

Dawson (G. M.), F.R.S. The Progress and Trend of Scientific Investigation in Canada. 4to. [*Montreal*] 1894. The Author.

Hayter (H. H.) Victorian Year-Book. 1893. Vol. II. 8vo. *Melbourne* 1894. The Author.

Jackson (B. D.) Index Kewensis: an Enumeration of the Genera and Species of Flowering Plants. Part 3. 4to. *Oxford* 1894. Sir J. D. Hooker, F.R.S.

Johnston (H. H.) Report on the Relation between Malarial Fever among Her Majesty's White Troops at Port Louis, Mauritius, and the Meteorological Elements of Temperature, Rainfall, and Relative Humidity for the Year 1890. 8vo. *Edinburgh* 1894; Thesis on the Relative Efficiency of certain Filters for removing Micro-organisms from Water, with special reference to the Nordtmeyer-Berkefeld and Pasteur-Chamberland Filters. 8vo. *Edinburgh* 1894. The Author.

Machado (V.) & Machado (A.) Chimica Geral e Analyse Chimica. 2 Vols. 8vo. *Lisboa* 1892. Royal Academy of Sciences, Lisbon.

Riccò (A.) & Arcidiacono (S.) Osservazioni Puteometriche eseguite nell' Osservatorio di Catania. 8vo. *Catania* 1894. The Authors

Riccò (A.) & Saija (G.) Confronto della Temperatura all' Osservatorio Etneo ed all' Osservatorio di Catania. 8vo. *Catania* 1894. The Authors.

Schreiber (P.) Ableitung der sogenannten Trägheitscurve auf der Erdoberfläche. 4to. *Dresden* 1894. The Author.

Smith (H. J. S.), F.R.S. Collected Mathematical Papers; edited by J. W. L. Glaisher, F.R.S. 2 vols. 4to. *Oxford* 1894. The Delegates, Clarendon Press, Oxford.

Spencer (J. W.) A Review of the History of the Great Lakes. 8vo. [*Minneapolis*] 1894. The Author.

Thomson, Sir William [Lord Kelvin], P.R.S. Popular Lectures and Addresses. Vol. I. Second edition. 8vo. *London* 1891. The Author.

Volterra (V.) Sur les Vibrations des Corps élastiques isotropes. 4to. [*Stockholm*] 1894. The Author.

November 30, 1894.

ANNIVERSARY MEETING.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

The Report of the Auditors of the Treasurer's Accounts, on the part of the Society, was presented as follows:—

"The total receipts on the General Account during the past year, including balances carried from the preceding year (£999 7*s*. 11*d*.) and the proceeds of the sale of stock, amount to £10,025 2*s*. 10*d*., and the total receipts on account of Trust Funds, including balances from the preceding year and cash received for bonds drawn, amounted to £6,065 8*s*. 2*d*. The total expenditure for the same period amounted to £7,227 11*s*. 10*d*. on the General Account, and £4,086 6*s*. 11*d*. on account of Trust Funds, leaving a balance on the General Account of £2,780 7*s*. 1*d*. at the bankers', which includes £500 Challenger Account, £1,700 Catalogue Account, and £247 8*s*. 6*d*. Water Research Account, and a balance of £17 3*s*. 11*d*. in the hands of the Treasurer; leaving also a balance at the bankers' on account of Trust Funds of £1,979 1*s*. 3*d*."

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary then read the following Lists :—

Fellows deceased since the last Anniversary (Nov. 30, 1893).

Royal.

H.R.H. Louis Philippe d'Orléans, Count of Paris.

On the Home List.

Armstrong, Robert Young, Col. R.E.
Baker, Sir Samuel White, M.A.
Bowen, Charles Synge C., Lord, D.C.L.
Brown-Séquard, Charles Édouard, M.D.
Coleridge, John Duke, Lord, D.C.L.
Hannen, James, Lord, D.C.L.
Hawkins, Rev. William Bentinck Latham, M.A.
Hodgson, Brian Houghton, D.C.L.
Inglefield, Sir Edward Augustus, Admiral, K.C.B.
Lovelace, William King, Earl of.
Marshall, Arthur Milnes, M.D.
Pengelly, William, F.G.S.
Romanes, George John, M.A.
Topley, William, F.G.S.
Tyndall, John, D.C.L.
Wright, Charles R. Alder, D.Sc.

On the Foreign List.

Beneden, Pierre J. van.
Helmholtz, Hermann Ludwig Ferdinand von.
Marignac, Jean Charles Galissard de.

Withdrawn or Removed.

Basing, George Sclater-Booth, Lord.
Thurlow, Thomas John Hovell-Thurlow Cumming-Bruce, Lord.

Fellows elected since the last Anniversary.

Bateson, William, M.A.
Boulenger, George Albert.
Bradford, John Rose, M.D.
Bryce, Right Hon. James.
Callendar, Prof. Hugh Longbourne.
Cheyne, Prof. William Watson, M.B., F.R.C.S.
Froude, Robert Edmund.
Hill, Prof. M. J. M., M.A., D.Sc.
Jones, Prof. John Viriamu, M.A., B.Sc.
Love, Augustus Edward Hough, M.A.
Lydekker, Richard, B.A.
Penrose, Francis Cranmer, M.A., F.R.A.S.
Scott, Dukinfield Henry, M.A., F.L.S.
Smith, Rev. Frederick John, M.A.
Swan, Joseph Wilson, M.A., F.I.C.
Veley, Victor Herbert, M.A., F.C.S.

On the Foreign List.

Baillon, Henri Ernest.
Poincaré, Henri.
Suess, Eduard.

The President then addressed the Society as follows:—

Since our last Anniversary Meeting, the Royal Society has lost eighteen Fellows and three Foreign Members.

H.R.H. Louis Philippe d'Orléans, Count of Paris, September 8, 1894, aged 56.
John Tyndall, December 4, 1893, aged 73.
The Earl of Lovelace, December 29, 1893, aged 89.
Sir Samuel White Baker, December 30, 1893, aged 72.
Arthur Milnes Marshall, December 31, 1893, aged 41.
Pierre J. Van Beneden, January 8, 1894, aged 93.
William Pengelly, March 16, 1894, aged 82.
Lord Hannen, March 29, 1894, aged 73.
Dr. Charles Édouard Brown-Séquard, April 1, 1894, aged 77.
Lord Bowen, April 10, 1894, aged 58.
Brian Houghton Hodgson, May 23, 1894, aged 94.
George John Romanes, May 23, 1894, aged 46.
Lord Coleridge, June 5, 1894, aged 74.
Charles R. Alder Wright, July 25, 1894, aged 50.
Rev. William Bentinck Latham Hawkins, August 31, 1894, aged 83.
Admiral Sir Edward Augustus Inglefield, September 5, 1894, aged 74.
Hermann Ludwig Ferdinand von Helmholtz, September 8, 1894, aged 73.
Jean Charles Galissard de Marignac, September 15, 1894, aged 77.
William Topley, October 2, 1894, aged 53.
Lord Basing, October 22, 1894, aged 68.
Colonel R. Y. Armstrong, November 1, 1894, aged 55.

Biographical notices will be found in the Proceedings.

Science has lost severely during the past year. In the list of Fellows deceased, which I have read to you, you have heard the names of Tyndall, Milnes Marshall, Van Beneden, Pengelly, Brown-Séquard, Romanes, Alder Wright, Helmholtz, Marignac, Topley, all well known to you as having been in their lives zealous and successful scientific investigators, who have largely contributed to the object for which the Royal Society works, "The Increase of Natural Knowledge." Tyndall, full of fire and enthusiasm in solid experimental work advancing the boundaries of science, contributed largely, by his brilliant lectures and books, to make science popular, as it now is in England and America. By the sad death of Milnes Marshall on Scawfell, in Cumberland, on the last day of 1893, we lost a young, able, and enthusiastic worker in zoology. A few months later, we lost the veteran Pengelly, who did so much for geological science, and gave such delightful and valuable lessons to the larger world of not scientific geologists, in what he did in his exploration of Kent's Cavern, Torquay. Romanes, full of zeal, fighting to the end with the most difficult problems that have ever occupied the mind of man, and devoting his health and his wealth to promote not merely philosophical speculation but also the experimental research by which alone philosophy can have a foundation, left us at the early age of 46.

A year ago, in my anniversary address, I called your attention to Hertz's experimental demonstration of electric waves, which he found in working out an experimental problem originally proposed by Helmholtz to him when he was engaged in experimental researches in the Physical Institute of Berlin in 1879. An English translation by Jones, of Hertz's book describing his work on electric waves, dedicated "with gratitude" to Helmholtz, was published in England and America in December, 1893. On the first day of the new year the disciple died, and within the year the master followed him. Of the whole of Helmholtz's great and splendid work in physiology, physics, and mathematics, I doubt whether any one man may be qualified to speak with the power which knowledge and understanding can give: but we can all appreciate, to some degree, the vast services which he has rendered to biology by the application of his mathematical genius and highly trained capacity for experimental research to physiological investigation.

In his interesting autobiographical sketch he tells us that his early natural inclination was for physics, which he found more attractive than purely geometrical and algebraic studies; but his father could only give him the opportunity of studying physics by his learning medicine to earn a livelihood, and he himself was by no means averse to thus entering on the study of living matter instead of confining himself to the physics of dead matter. I think we may now feel that the world has gained largely by this early necessity for a young man of great genius and power to choose a practical profession.

One early result was his careful examination, while still a student, of the theory of animal heat, and a little later (1847) his great essay, 'Ueber die Erhaltung der Kraft,' Conservation of Energy as we now call it, communicated to the Physical Society of Berlin on the 3rd July, 1847, of which he said in 1891, "My aim was merely to give a critical investigation and arrangement of the facts for the benefit of physiologists." As a student he had found that Stahl's theory, ascribing to every living body the possession of the property of "The Perpetual Motion" as an essence of its "Vital force," was still held by most physiologists. His essay on the "Conservation of Energy," giving strong reasons for rejecting that theory, though looked upon, at first, by many of the physical and philosophical authorities of the time as a fantastic speculation, was enthusiastically welcomed by younger student-philosophers, and must soon have convinced the elder men that, whatever may be the real efficiency of vitality, vast and wonderful as it is, it does *not* include the performance of work without drawing upon a source of energy. This conclusion had been virtually foreseen before the end of last century by Rumford and Davy, and had been clearly stated and powerfully supported by Joule and Mayer a few years before Helmholtz found it for himself and successfully persuaded others of its truth.

It is interesting for us now to know that, while thus contributing so effectively to the abandonment of the old doctrine that vital "force" can work without drawing on an external source of energy, Helmholtz was even more effectively concerned in the establishment of a new doctrine which has given a vast extension to the province of life, previously perhaps undreamt of, but now universally recognised as thoroughly well established, and supremely important in modern physiology and medicine. On recovering from a typhus fever in the autumn of 1841, at the age of 20, the last year of his undergraduate course in the Army Medical School of the Friedrich Wilhelm's Institute, he spent the accumulations of his income, which free treatment at the hospital during his illness had left him, in the purchase of a microscope, an instrument then but little used in medical education. He began immediately to use it, and made some important observations on the ganglion cells of invertebrates, which, at the suggestion of his master, Johannes Müller, he took as the subject of his inaugural thesis for the doctor's degree, in November, 1842, and which was his first published work.* With the same microscope, he observed vibrios in putrefying liquids, which he described in his second published paper (1843), "On the Nature of Putrefaction and Fermentation." His distinguished comrade, Schwann, in the laboratory of Johannes Müller, had already shown that vegetable cells are present in fermenting solutions of sugar, and that air, which had been highly heated, was incapable of exciting the fermentation which the access of ordinary atmospheric air was known to produce. Helmholtz found that oxygen, yielded by the decomposition of water in flasks containing small pieces of boiled meat, did not produce putrefaction. Thus the doctrine, held perhaps by all before them, and certainly supported by the great Liebig, that putrefaction and fermentation are purely chemical processes of eremacausis (or slow combustion), produced by oxygen, was thoroughly disproved by the two young investigators. But Helmholtz went farther, and showed almost certainly that the actual presence of a living creature, vibrio, as he called it, bacterium, as we more commonly call it now, is necessary for either fermentation or putrefaction. He proved by experiment that a partition of moist bladder, between the yeast and the fermentable liquid, prevented the entrance of the vibrios which he had observed, *and prevented the fermentation.* It had been reasonably suggested that fermentation or putrefaction might be a purely chemical process produced by a quasi-chemical agent or poison secreted by a living organism; but Helmholtz's observation disproved this supposition almost certainly, because any such chemical substance in solution would pass by diffusion through the bladder, and produce its effect without any direct action of the living creatures. Although Helmholtz himself was characteristically philosophical and conscientious in not claiming, as absolutely proved, what he had only rendered probable, it is certain that this early work of his on putrefaction and fermentation constituted a very long step towards the great generalisation of Pasteur, adverse to spontaneous generation, and decisive in attributing to living creatures, born from previous living creatures, not only fermentation and putrefaction, but a vast array of the virulent diseases and blights, which had been most destructive to men, and the lower animals and crops and fruits. It is well that Helmholtz himself lived to see the great benefits conferred on mankind by Pasteur's work; and by the annulment of the deadliness of compound fractures and the abolition of hospital gangrene in virtue of Lister's antiseptic treatment; and by the sanitary defences against fevers and blights, realised by many other distinguished men as practical applications of the science which his own typhus fever of 1841 helped so much to create.

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* Helmholtz's 'Wissenschaftliche Abhandlungen,' vol. 2, p. 663.

Close after his work on this subject and on animal heat, followed investigations on the velocity of transmission along the sensory nerves of the disturbance to which sensation is due, the time which the person perceiving the sensation takes to decide what to do in consequence, and the velocity of transmission of his orders along the motor nerves to the muscles which are to carry out his will. Results of the highest scientific interest and of large practical importance were given in two great papers published in 1850.* These were followed a few years later by his "Tonempfindungen," a great work, not merely confined to the perception of sound, but including mathematical and experimental investigations on the inanimate external influences concerned in sound, investigation of the anatomical structure of the ear in virtue of which it perceives sound, and applications to the philosophical foundation of the musical art, which holds a unique position in the literature of philosophy, and is certainly a splendid monument to the genius and indomitable working power of its author. Another great work of Helmholtz is his "Physiologische Optik;" who shall say which of the two books is the more important, the more interesting, or the more valuable? Each of them has all these qualities to a wonderfully high degree.

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* Helmholtz's 'Wissenschaftliche Abhandlungen,' p. 763—861.

Perhaps the most interesting of his experimental investigations in physiological optics was the measurements, by his ophthalmometer, of the curvatures of the several refracting surfaces constituting the lens-system of the eye, from which he ascertained that it is almost altogether by changing the curvature of the front surface of the crystalline lens that the eye is accommodated by its possessor to vision at different distances. His ophthalmoscope, by which for the first time he himself saw and showed to others the retina of the living eye, was a splendid and precious contribution to medicine. By allowing that outlying portion of the brain to be distinctly seen and examined, it has shown the cause of many illnesses which had been regarded as hopelessly obscure; and for diagnosis and guidance of medical treatment, it is now continually used not only by oculists, but by general practitioners.

Constrained as I feel not to overtax your patience, I find it impossible on the present occasion, to enter upon Helmholtz's researches in mathematics and mathematical physics farther than just to mention his small but exquisite paper on anomalous dispersion, and the grand contribution to hydrodynamics which we have in his "Integrals of the Hydrodynamical Equations which express Vortex Motion."*

Since our last anniversary, important questions regarding the conduct of the ordinary meetings and the publication of papers, both in the 'Transactions' and 'Proceedings' of the Royal Society, have been engaging the attention of the Council, with the assistance of a Committee appointed on the 5th July, 1893. The final report of this Committee was submitted to the Council on the 5th July, 1894, when resolutions were adopted accepting some of its recommendations and deferring the consideration of others until after the recess.

At the request of the Royal Geographical Society, a Committee was appointed by the Council of the Royal Society to consider the advisability of asking the Government to undertake an Antarctic Expedition. A very important and valuable Report on the advantages which such an expedition would bring, both to science and to practical navigation, was presented by this Committee to the Council on the 24th May. The Council, after much careful consideration, resolved to ask the Lords of the Admiralty to grant an interview on the subject with representatives of the Royal Society. This request was assented to: and an interview was accordingly held between the First Lord of the Admiralty and representatives of the Royal Society; but the proposal of an Antarctic Expedition was not favourably received.

The Joule Fund Committee submitted its report on the 7th

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* 'Philosophical Magazine,' July, 1867, being the translation by Tait of the original German paper, which appeared in Crelle's Journal in 1858, and which has been republished in 'Wissenschaftliche Abhandlungen,' vol. 1, pp. 101—134.

December, 1893, and the Council, on its recommendation, adopted the following resolutions:—

I. That the Regulations for administering the Joule-Memorial Fund be as follows:—

(1) That the proceeds be applied in the form of a studentship or grant, to be awarded every second year, to assist research, especially among younger men, in those branches of physical science more immediately connected with Joule's work.

(2) That this grant be international in its character, and awarded alternately in Great Britain and abroad, or in such order as the President and Council shall from time to time decide.

(3) That it be awarded in Great Britain by the President and Council of the Royal Society; and, for award in France, offered to the Académie des Sciences, Paris; and in Germany to the K. Akademie der Wissenschaften, Berlin; or, in any other country, to the leading scientific institution, for award in that country.

(4) That the award in Great Britain be made on the recommendation of a Committee, from time to time appointed by the President and Council of the Royal Society, but not of necessity confined to Fellows of the Society.

II. That a sum of £100, which is now, or shortly will be, available, for the first studentship or grant be awarded in accordance with Regulation 4.

The first appointment was accordingly made on the 21st June, 1894, when it was resolved:—

(1) "That a Joule Scholarship of the Royal Society Memorial Fund be awarded to Mr. J. D. Chorlton, of Owens College, Manchester, for the purpose of enabling him to carry on certain researches on lines laid down by Dr. Joule, more especially with the view of determining the constants of some of the instruments employed by Dr. Joule, which can be placed at his disposal by his representatives."

(2) "That the value of the Scholarship be £100, payable quarterly, on the certificate from the authorities of Owens College that the researches are being conducted in a satisfactory manner."

On the occasion of Sir George Buchanan's retirement from the post of Chief Medical Officer to the Local Government Board, it was decided by some of his friends that a testimonial should be presented to him, and a sum, amounting to about £340, has been subscribed by medical officers of health, sanitary engineers, and others interested in sanitary science. It was resolved, on the suggestion of

Sir George Buchanan himself, that this testimonial should take the form of a medal, to be awarded periodically for work done in connection with sanitary science, and that the Royal Society should be asked to administer the testimonial fund under the following conditions:—

1. The money collected, after paying expenses incurred, to be devoted—

   (*a*) To the foundation of a Gold Medal of the value as nearly as may be of twenty guineas, with a portrait of Sir George Buchanan on the one side and an appropriate design on the other, to be awarded every three or five years in respect of distinguished services to Hygienic Science or Practice, in the direction either of original research or of professional, administrative, or constructive work.

   (*b*) To the bestowal on the recipient of the Medal of the amount (remaining after paying for the Medal and discharging the incidental expenses) which has accumulated since the last award.

2. The Medal to be awarded without limit of nationality or sex.

The Council of the Royal Society has accepted the Trust under these conditions; and it was agreed that the first medal should be given to Lady Buchanan by the testimonialists themselves.

The Catalogue Department has been specially active in the past session. Mr. Ludwig Mond's generous gift of £2000, which I announced to the Society in my Anniversary Address last year, has given a new impulse to our operations in that department, and enabled us to increase the staff of assistants. Under the able superintendence of Miss Chambers, Volume 10 of the Catalogue under authors' names has been completed, and was issued in June of the present year. The Society is indebted to several members of the Catalogue Committee who have lent their scientific knowledge to aid in the revision of the proofs, and especially to the Treasurer, under whose experienced eye every sheet in the Catalogue has passed. The preparation of copy for a supplementary volume, which will include papers from a large number of periodicals not included in the existing volumes, is now nearing completion.

The Catalogue Committee have held several meetings and discussed some important questions. The proposed subject-index to the existing Catalogue has been the chief matter under consideration, and the burning question of the respective merits of an alphabetical and a classified index has been so far settled as to make it possible to commence the work of transcription and translation, nearly 40,000 slips being already finished, so that when the details of the plan agreed upon have been finally settled, as there is good hope they will be in the near future, the preparation of the copy for the printer can be speedily proceeded with. Before, however, any final steps can be taken, it will be necessary that the supplement volume of the catalogue should have issued from the press. The preparations for this volume are in active progress.

A kindred subject, but one of still wider scope, has been discussed by a Special Committee appointed by the Council at their first meeting in the present session. The question, namely, of a scientific subject-catalogue, which it is proposed to carry out by means of international co-operation. This Committee, with the sanction of the Council, have addressed a circular letter to scientific societies and institutions in this country and abroad, proposing by way of preliminary suggestions, first, that the Catalogue should commence with the next century; secondly, that a central office or bureau should be maintained by international contributions; and third, that this office should be supplied with all the information necessary for the construction of the Catalogue. The circular invites the views on this subject of scientific bodies and scientific men, without in any way committing the Society to farther action. A large number of replies to this circular have been received, many of them carefully prepared and able documents. They will be submitted to the new Council of the Royal Society, and will, I am sure, be most valuable in assisting it to judge as to future proceedings.

The principal question which the Library Committee have had before them during the past session is the accumulation of the stock of 'Philosophical Transactions' from the beginning of the century to the present time. New racks have been erected in the basement which have partly relieved the pressure on our space, but the Committee recognise the necessity of some active measures being taken to increase the sale of this accumulated stock. They are of opinion that the sale might be much facilitated if the memoirs composing the volumes published in the past were made separately available to the public, as is done with those that are published at the present time. On the advice of the Committee, the Council have empowered the Treasurer to treat with one of the leading booksellers with the view of bringing some such arrangement into effect.

The collection of marble busts belonging to the Society, which is of such personal and historical interest to all our Fellows, has received most important and valuable accessions. The sons of our former President, Mr. William Spottiswoode—Messrs. Hugh and Cyril Spottiswoode—have presented to the Society a marble bust of their father, by Woolner, which will find in our apartments a fitting home among the busts of many of our former Presidents and distinguished Fellows, and will hand down to posterity a striking likeness of one who deserved so well of the Society and whose premature decease we all still deplore. Earlier in the session, Mr. Alfred W. Dollond presented a marble bust by Garland, of his great-uncle, George Dollond, F.R.S., who himself presented a bust of John Dollond, in 1843, by the same sculptor.

The House and Soirée Committee have discussed the advisability of increasing the accommodation in the tea room, and have presented a report to the Council upon the subject. The Council, while not disagreeing with this report, considered it wiser, in the present state of finances, to defer the matter for a time.

A third Report of the Water Research Committee has been issued during the present year. It gives the results of further experiments by Professor Marshall Ward on the "Action of Light on Bacillus Anthracis," and on the "Bacteria of the Thames," and the experiments of Professor Percy Frankland on the "Behaviour of the Typhoid Bacillus and of the *Bacillus Coli Communis* in Potable Water," the whole filling 242 octavo pages.

Unusually large as was the amount of matter published last year, this year the amount is even larger. In the mathematical and physical section of the 'Philosophical Transactions,' seventeen papers have been published, eighteen in the biological section. The two sections together contain, in all, 1992 pages of letterpress, and 112 plates; to which must be added eight or ten papers now passing through the press, and probably to be issued before the close of the year. Of the 'Proceedings,' ten numbers have been issued, containing 1026 pages. As a result, the finances of the Society are, I regret to say, in not such a satisfactory condition as could be desired. The cost of the publications, which, last year, was far in excess of what it was in previous years, and of what the Society could really afford, has, in the year 1894, amounted to nearly £3260, or about £90 more than it was in 1893. For lithography and engraving alone £1516 have been paid, as against £977 last year. There is, moreover, an accumulation of printed matter now almost in readiness to be issued, the cost of which has still to be defrayed. To meet this extraordinary expenditure it has been necessary to sell out enough of the Society's funded capital to produce £1000, and rigorous retrenchment will be necessary in order to avoid further loss of provision for continued work in future. While the Council feels the importance of all the publications of the Society being as completely illustrated and as fully detailed as the subjects discussed may require, it is evident that some check must be placed on the extent of the publications, and the best manner of effecting this end is occupying the careful attention of the Council.

The establishment of the Faraday-Davy Research Laboratory, in connexion with the Royal Institution, is a splendid benefaction which science has gained during the past year, through the untiring and grand generosity of Mr. Ludwig Mond. The Royal Society interests itself in all work contributing towards the object for which it was founded—the increase of natural knowledge; and while gratefully remembering the assistance so generously given to it in the humble but highly valuable work of cataloguing papers which describe the results of scientific investigations already made, it hails with delight this grand foundation of a practical laboratory, of which the purpose is not the teaching of scientific truths already discovered, but the conquering of fresh provinces from the great region of the unknown in Nature.

The greatest scientific event of the past year is, to my mind, undoubtedly the discovery of a new constituent of our atmosphere. If anything could add to the interest which we must all feel in this startling discovery, it is the consideration of the way by which it was found. In his Presidential address to Section A of the meeting of the British Association at Southampton in 1882, Lord Rayleigh, after calling attention to Prout's law, according to which the atomic weights of the chemical elements stand in simple relationship to that of hydrogen, said:—"Some chemists have reprobated strongly the importation of *à priori* views into the consideration of the question, and maintain that the only numbers worthy of recognition are the immediate results of experiment. Others, more impressed by the argument that the close approximations to simple numbers cannot be merely fortuitous, and more alive to the inevitable imperfections of our measurements, consider that the experimental evidence against the simple numbers is of a very slender character, balanced, if not outweighed, by the *à priori* argument in favour of simplicity. The subject is eminently one for further experiment; and as it is now engaging the attention of chemists, we may look forward to the settlement of the question by the present generation. The time has, perhaps, come when a re-determination of the densities of the principal gases may be desirable—an undertaking for which I have made some preparations." The arduous work thus commenced in 1882, has been continued for 12 years,* by Rayleigh, with unremitting perseverance. After 11 years of it, a first important part of the object, the determination of the atomic weight of oxygen with all possible accuracy was attained by the comparison,[*] of Scott's determination of the ratio of the volumes of hydrogen and oxygen in the constitution of water, with Rayleigh's determination of the ratio of the densities. The result was 15·82, which is almost 1 per cent. (0·87 per cent.) less than the 16, which it would be according to Prout's law. It is very slightly less ($\frac{1}{4}$ per cent.) than Dittmar and Henderson's value obtained by an investigation[†] for which the Graham medal of the Glasgow Philosophical Society was awarded in 1890. Values, not quite so small as these for the atomic weight of oxygen, had been previously found by Cooke and Richards (15·869) and by Leduc (15·876). There can be no doubt whatever now that the true value is more than $\frac{1}{2}$ per cent. smaller than according to Prout's law, and that in all probability it agrees exceedingly closely with the results obtained by Rayleigh and Scott, and by Dittmar and Henderson. The question of Prout's law being thus so far set at rest, Rayleigh, persevering in the main object which he had promised in 1882, "a redetermination of the densities of the principal gases," attacked nitrogen resolutely and, stimulated by most disturbing and unexpected difficulties in the way of obtaining concordant results for the density of this gas as obtained from different sources, discovered that the gas left by taking vapour of water, carbonic acid, and oxygen from common air was denser[‡] by 1/230 than nitrogen obtained by chemical processes from nitric oxide or from nitrous oxide, or from ammonium nitrite, thereby rendering it probable that atmospheric air is a mixture of nitrogen and a small proportion of some unknown and heavier gas. Rayleigh, and Ramsay who happily joined in the work at this stage, have since succeeded in isolating the new gas, both by removing nitrogen from common air by Cavendish's old process of passing electric sparks through it, and taking away the nitrous compounds thus produced by alkaline liquor; and by absorption by metallic magnesium. Thus we have a fresh and most interesting verification of a statement which I took occasion to make in my Presidential address to the British Association in 1871,[§] "Accurate and minute measurement seems to the non-scientific imagination a less lofty and dignified work than looking for something new. But nearly all the grandest discoveries of science have been but the rewards of accurate measurement and patient long-continued labour in the minute sifting of numerical results." The investigation is now being carried on vigorously, and has already led to the wonderful conclusion that it does not combine with any other chemical substance which has hitherto been presented to it. We all wait with impatience for further results of the work; we wish success to it, and we hope that it will give us, before the next anniversary meeting of the Royal Society, much knowledge of the properties, both physical and chemical, of the hitherto unknown and still anonymous fifth constituent of our atmosphere.

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* "On the relative Densities of Hydrogen and Oxygen. Preliminary Notice," by Lord Rayleigh, February 2, 1888.

"On the Composition of Water," by Lord Rayleigh, February 26, 1889.

"On the relative Densities of Hydrogen and Oxygen. II." By Lord Rayleigh, February 5, 1892.

"On the Densities of the principal Gases," by Lord Rayleigh, March 23, 1893.

"On an Anomaly encountered in Determinations of the Density of Nitrogen Gas," by Lord Rayleigh, April 19, 1894.

All published in the 'Proceedings of the Royal Society.'

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[*] Scott, "On the Composition of Water by Volume," communicated by Lord Rayleigh, 'Roy. Soc. Proc.,' March 23, 1893.

[†] 'Proceedings of the Philosophical Society of Glasgow,' 1890—1891.

[‡] "On an Anomaly encountered in Determinations of the Density of Nitrogen Gas," 'Roy. Soc. Proc.,' April, 1894.

[§] Republished in Volume 2 of 'Popular Lectures and Addresses.'

COPLEY MEDAL.

Dr. Edward Frankland, D.C.L., F.R.S.

The Copley Medal is awarded to Dr. E. Frankland for his eminent services to theoretical and applied chemistry.

At a time when the classification of organic compounds in homologous series was a comparative novelty, when isomerism was still a profound mystery, and the theory of compound radicles introduced by Liebig was still on its trial, Dr. Frankland made his first attempt (in 1848) to isolate the radicle of common alcohol. Though the attempt was in one sense unsuccessful, inasmuch as the free radicle was never obtained, for reasons which we now more fully understand, the research led to important consequences. The discovery of the organo-metallic compounds, and the study of their composition and properties, was followed by a recognition of the fact, first that the capacity for combination possessed by the atoms of the metals was limited ('Phil. Trans.,' 1852), and secondly that variation of "atomicity," as it was then called, usually occurs by an even number of units ('Journ. Chem. Soc.,' 1866), represented by atoms of hydrogen, chlorine, or such compound radicles as methyl, ethyl, and the rest. These discoveries form the basis of the modern doctrine of valency, with all the important consequences that follow, including the idea of the orderly linking of atoms, and hence the theories of structure or constitution now current.

The discovery of zinc ethyl placed in the hands of chemists an important new instrument of research, which Dr. Frankland was himself the first to use in his investigations concerning the synthetical production of acids of the lactic and acrylic series. Further important synthetical work, conducted in concert with Mr. Duppa, led to a method of ascending the series of acids homologous with acetic acid.

Dr. Frankland's researches in pure chemistry are almost rivalled in interest by his discoveries in physical chemistry, especially in relation to the influence of pressure on the rate of combustion, on the light emitted during combustion, and on the cause of luminosity in hydrocarbon flames.

The important work done by Dr. Frankland in the study of water-supply and sewage, and illuminating gas, has proved of great practical value, and has rendered his name famous in connection with the application of chemistry to technical purposes.

RUMFORD MEDAL.

Professor Dewar.

During more than twenty years past Professor Dewar has been engaged in researches of great difficulty, in the first instance at very high, and latterly at very low temperatures, his inquiries having extended over an extraordinarily wide field, as will be seen by reference to the 'R.S. Catalogue' of scientific papers.

In conjunction with Professor Liveing, he has communicated to the Royal Society a large number of papers which have added much to our knowledge of spectroscopic phenomena.

During recent years he has made the liquefaction of gases a subject of deepest study, and in the course of this work has displayed not only marvellous manipulative skill and fertility of resource, but also great personal courage, such researches being attended with considerable danger. One of his chief objects has been so to improve and develope the methods of liquefying the more permanent gases that it shall become possible to deal with large quantities of liquid, and to use such liquids as instruments of research in extending our knowledge of the general behaviour of substances at very low temperatures. In this he has already been highly successful. Not only has he succeeded in preparing large quantities of liquid oxygen, but he has been able by the device of vacuum-jacketed vessels to store this liquid under atmospheric pressure during long periods, and thus to use it as a cooling agent. Very valuable outcome of these labours has been the series of determinations, made by him in conjunction with Dr. Fleming, of the electrical conductivity of metals at exceedingly low temperatures, which have furnished results of a most unexpected character, and of extraordinary interest and importance. Professor Dewar's experiment showing the great magnetic susceptibility of liquid oxygen is exceedingly important and interesting. His recent observations on phosphorescence, and on photography,* and on ozone† at very low temperatures, have given surprising results of a highly instructive and interesting character. It is difficult to exaggerate the importance of extending these researches, which certainly deserve all possible encouragement and support. The award of the Rumford Medal to Professor Dewar is made in recognition of the services which he has rendered to science by the work which he has already done and the provision he has been successful in making for future work, in the investigation of properties of matter at lowest temperatures.

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* 'Chem. Soc. Proc.,' June 28, 1894.

† 'Phil. Mag.,' August, 1894, pp. 238, 239.

ROYAL MEDAL.

Professor J. J. Thomson, F.R.S.

Professor J. J. Thomson has distinguished himself in both mathematical and experimental fields of work. His first essay on vortex rings showed power of grappling with difficult problems, and added to our knowledge concerning the encounter of rings which came within a moderate distance of one another so as to deflect each others' paths.

His theoretical work in the borderland of chemistry and physics has been very interesting and suggestive. His experimental work has likewise been mainly on the borders of chemistry and physics. He has observed the large conductivity of many gases and vapours, and proved the non-conducting power of several others, founding on the conducting power of iodine vapour important speculations as to its probable chemical constitution.

He has also measured the specific resistance of various electrolytes, under extremely rapid electric oscillations, by an ingenious and valuable method, based on the partial opacity of semi-conducting matter to electro-magnetic waves. Recently he has worked at the discharge of electricity through rarefied gases, getting induced currents in closed circuits in sealed bulbs without electrodes, and, in especial, measuring to a first approximation the absolute velocity of the positive discharge through a long vacuum tube, proving that it was comparable with, though decidedly less than, the velocity of light. He also gave an ingenious theory of the striæ—a theory which he has since endeavoured, with some success, to extend to a large number of electrical phenomena, the whole of electric conduction and induction being regarded by him from the chemical side as a modified or incipient electrolysis, or as concerned with electrolytic chains of molecules or "Faraday tubes."

Some of his recent mathematical work on the theory of electric oscillations in spheres and cylinders, and in dumb-bell oscillators of the kind used by Hertz, with reference to not only their oscillation-frequency but also their damping efficiency, has been of much service to experimental workers in those branches of physics. And, in general, the effective manner in which he attacks any electrical problem presenting itself, as evidenced by his book on Recent Researches in Electricity and Magnetism, wherein he worthily carries on into a third volume the great treatise begun by Clerk Maxwell, is evidence of consummate ability combined with remarkable energy and power of work.

ROYAL MEDAL.

Professor Victor Horsley, F.R.S.

A Royal Medal is awarded to Professor Victor Horsley, F.R.S., for his laborious and fruitful researches in physiology and pathology, and particularly for those relating to the functions of the nervous system and of the thyroid gland. His inquiries relating to the former subject have been pursued for more than ten years, and have been communicated to the Royal Society in a succession of papers, the most important of which have been published in the 'Philosophical Transactions.' The first of the series of researches ('Phil. Trans.', 1888), which was conducted in co-operation with Professor Schäfer, and concerned the relation of a part of the cerebral cortex (the limbic lobe) to sensation, afforded a new confirmation and extension of the doctrine of the localisation of cerebral function now generally accepted. While this work was in progress, Professor Horsley engaged with Dr. Beevor in a long and laborious series of experiments, for the purpose of determining with the utmost attainable accuracy the nature of the muscular responses which are evoked by stimulating the convolutions in the quadrumana. The results of these researches were communicated in four papers, of which the first three relate to the "cortical representations" of the movement of the limbs, and of those of the tongue and face ('Phil. Trans.,' 1887—1890); the fourth to the channels (in the internal capsule) by which the cortex exercises its influence on the rest of the nervous system ('Phil. Trans.,' 1890).

These experiments not only served to bring to light a number of new facts, and to elucidate their physiological relations in a very remarkable way, but had a special interest in their bearing on the physiology and pathology of the brain in man. Their importance in this respect is enhanced by the circumstance that in the course of the enquiry the opportunity offered itself of comparing the brain of the monkey with that of the orang ('Phil. Trans.,' 1890), a brain which so closely approaches that of man in its structure that the knowledge acquired by these researches may now be confidently used as a guide in the diagnosis and treatment of cerebral disease. Professor Horsley has himself shown—and this is not the least of the merits which it is desired to recognise in the bestowal of the Royal Medal—in how many instances the knowledge which is acquired by patient and skilful work in the laboratory may be made available for the saving of life, or the alleviation of human suffering.

In connection with this leading series of researches, two others relating to the physiology of the central nervous system must be referred to. In one of these ('Phil. Trans.,' 1890), Professor Horsley (in co-operation with Dr. Semon) established the existence, not only of a co-ordinating centre in the bulb, but of a cortical area in physiological relation with the respiratory and phonatory movements of the larynx; in the other, in conjunction with Professor Gotch, he investigated the electrical changes in the spinal cord which are associated with excitation of the cortex and internal capsule, and showed how the observation of these facts can be made available for tracing channels of conduction in the cord.

As regards the thyroid gland, Professor Horsley's inquiries relating to functions of that organ were, like those relating to the nervous system, begun ten years ago, though the results were not communicated to the Royal Society until three years later. Their purpose was to ascertain the nature of the very marked influence which the thyroid was known to exercise on the nutritive functions of the organism, and to show that this influence is constant and definite. In this field, Professor Horsley has not only the merit of having been one of the earliest workers, but of having at this early period arrived at results which the numerous investigations of subsequent writers have in all essential particulars confirmed.

DAVY MEDAL.

Professor Peter Theodor Cleve.

The Davy Medal is awarded to Peter Theodor Cleve, Professor of Chemistry in the University of Upsala, for his services to chemical science during the last thirty years, and in particular for his long-continued and valuable researches on the chemistry of the rare earths.

This field of inquiry is pre-eminently Scandinavian. By the manner in which he has cultivated it, Professor Cleve has shown himself a worthy successor of such forerunners as Gadolin, Berzelius, and Mosander, and by sound and patient investigation he has faithfully upheld the traditions inseparably associated with these names. All chemists are agreed that no department of their science demands greater insight or more analytical skill than this particular section. Many of the minerals which furnish the starting point for investigation are extremely rare, and the amounts of the several earths which they contain are frequently very small. Moreover, the substances themselves are most difficult of isolation, and their characters are so nearly allied that the greatest care and judgment are required in order to determine their individuality.

A remarkable example of Professor Cleve's power in overcoming these difficulties is seen in his masterly inquiry into the affinities and relations of the element scandium, discovered by Nilson. This, one of the rarest of the metals, is found only in gadolinite to the extent of 0·003 per cent., and in yttrotitanite to the extent of about 0·005 per cent. The whole amount of the material, as oxide, at Cleve's disposal was only about 1 gram, but with this small quantity he determined the atomic weight of the element, and ascertained the characters of its salts with such precision as to leave no doubt of the identity of scandium with the element *Ekabor*, the existence of which was predicted by Mendeleef, in the memorable paper in which he first ennunciated the Law of Periodicity. Cleve's research, indeed, constitutes one of the most brilliant proofs of the soundness of the great generalisation which science owes to the Russian chemist.

A not less remarkable instance of Cleve's skill as a worker is seen in his research on samarium and its compounds, which he communicated, as one of its Honorary Foreign Fellows, to the Chemical Society of London. The existence of samarium was inferred independently by Delafontaine and Lecoq de Boisbaudran, but we owe to Cleve the first comprehensive investigation of its characters and chemical relations. From the nature of its compounds, a large number of which were first prepared and quantitatively analysed by Cleve, and from the value of its atomic weight, which was first definitely established by him, it would appear that samarium most probably fills a gap in the eighth group of Mendeleef's system.

We are further indebted to Cleve for a series of determinations of the atomic weights of the rare substances yttrium, lanthanum, and didymium; these are generally accepted as among the best authenticated values for these particular bodies.

No record of Cleve's scientific activity would be complete without some reference to his investigations in the domain of organic chemistry, and more particularly to his studies, extending over twenty years, of naphthalene derivatives. By these researches, made partly independently, and partly in conjunction with his pupils, among whom may be named Atterberg, Widman, Forsling, and Hellström, Cleve has gradually brought order out of confusion, and has supplied most valuable experimental evidence of the constitution of naphthalene, and of the course of substitution of naphthalene derivatives. Within recent years a score of workers have occupied themselves with the same field of research, and no greater proof of Cleve's accuracy and care as an investigator could be furnished than the manner in which his naphthalene work—confessedly one of the most intricate and complicated sections of the chemistry of aromatic compounds—has stood the ordeal of revision.

DARWIN MEDAL.

Rt. Hon. T. H. Huxley, F.R.S.

The Darwin Medal is awarded to Thomas Henry Huxley.

Of Mr. Huxley's general labours in biological and geological science I need say nothing here. They are known of all men, and the Society showed its appreciation of their worth when it awarded to him the Copley Medal in 1888. The present medal is a token of the value put by the Society on the part of his scientific activity bearing more directly on the biological ideas with which the name of Charles Darwin will always be associated.

All the world now knows in part, no one perhaps will ever know in full, how, in the working out of his great idea, Darwin was encouraged, helped, and guided by constant communion with three close and faithful friends, Charles Lyell, the younger Joseph Dalton Hooker, and the still younger Thomas Henry Huxley. Each representing more or less different branches of science, each bringing to bear on the problems in hand more or less different mental characters, all three bore share, and were proud to bear share, in aiding the birth of the "Origin of Species." Charles Lyell has long been removed from us. Two years ago it was my pleasing duty to place the Darwin Medal in the hands of Joseph Dalton Hooker; that pleasing duty is renewed to-day in now giving it to the last of the three "who kept the bridge."

To the world at large, perhaps, Mr. Huxley's share in moulding the thesis of "Natural Selection" is less well known than is his bold unwearied exposition and defence of it after it had been made public. And, indeed, a speculative trifler, revelling in problems of the "might have been," would find a congenial theme in the inquiry how soon what we now call "Darwinism" would have met with the acceptance with which it has met, and gained the power which it has gained, had it not been for the brilliant advocacy with which in its early days it was expounded to all classes of men.

That advocacy had one striking mark; while it made or strove to make clear how deep the new view went down and how far it reached, it never shrank from striving to make equally clear the limits beyond which it could not go. In these latter days there is fear lest the view, once new but now familiar, may, through being stretched farther than it will bear, seem to lose some of its real worth. We may well be glad that the advocate of the "Origin of Species by Natural Selection," who once bore down its foes, is still among us, ready, if needs be, to "save it from its friends."

The Statutes relating to the election of Council and Officers were then read, and Professor Armstrong and Admiral Sir Erasmus Ommanney having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—The Lord Kelvin, D.C.L., LL.D.

Treasurer.—Sir John Evans, K.C.B., D.C.L., LL.D.

Secretaries.— { Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Sir Joseph Lister, Bart., F.R.C.S.

Other Members of the Council.

Andrew Ainslie Common, LL.D.; William Crookes, F.C.S.; Francis Darwin, M.A.; Andrew Russell Forsyth, Sc.D.; Sir Douglas Galton, K.C.B.; Professor Alexander Henry Green, M.A.; Sir John Kirk, K.C.B.; Professor Horace Lamb, M.A.; Professor Edwin Ray Lankester, M.A.; Professor Alexander Macalister, M.D.; Professor John Henry Poynting, D.Sc.; Professor Arthur William Rücker, M.A.; Osbert Salvin, M.A.; Professor J. S. Burdon Sanderson, M.D.; Thomas Edward Thorpe, Sc.D.; William Henry White, C.B.

The thanks of the Society were given to the Scrutators.

Balance Sheet. 1894.

***Statement of Receipts and Expenditure from November* 12th, 1893, *to November* 12th, 1894.**

	£	s.	d.	£	s.	d.
To Balance at Bank, 12th November, 1893				2,875	16	6
„ Balance in hand, Catalogue Account	3	3	3	10	3	4
„ „ Petty Cash	7	0	1			
„ Compositions				60	0	0
„ Admission Fees				10	0	0
„ Annual Contributions, 121 at £4	£484	0	0	1,072	0	0
„ 196 at £3	588	0	0			
„ Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions				342	0	0
„ Rents:	£	s.	d.			
Fee Farm, Lewes	18	12	0	95	13	10
Mablethorpe Estate	77	1	10			
„ Ground Rents				600	11	9
„ Dividends (exclusive of Trust Funds)				2,055	3	11
„ Interest on Mortgage Loans (Duke of Norfolk)				515	7	10
„ Sale of Transactions and Proceedings				678	0	1
„ Interest on Bank Deposit Account				25	18	10
„ Interest on Bank Deposit (Catalogue Account)				25	18	10
„ Sale of Catalogue				22	17	8
„ Transfer from Handley Fund on account of Catalogue				186	2	4
„ Sale of Krakatoa Report (leaving £62 10*s.* 4*d.* Expenditure in excess of Receipts)				....	....	....
„ Sale of Lendenfeld Monograph (leaving £664 19*s.* 1*d.* Expenditure in excess of Receipts)				....	....	....
„ Water Research Grants				250	0	0
„ Kew Committee—Repayment of Loan				200	0	0
„ Proceeds of Sale of £722 14*s.* 1*d.* India 3 per Cent. Stock				724	9	2
„ Ditto, £270 11*s.* 11*d.* 2¾ per Cent. Consolidated Stock				274	18	9
				£10,025	2	10

	£	s.	d.	£	s.	d.
By Salaries and Wages				1,443	10	4
„ Catalogue of Scientific Papers and Index				754	5	8
„ Books for the Library				321	9	10
„ Printing Transactions, and Separate Copies to Authors and Publisher	446	1	10	3,259	2	2
„ Ditto Proceedings, Nos. 327 to 337	538	10	11			
„ Ditto Miscellaneous	191	5	3			
„ Paper for Transactions and Proceedings	478	2	8			
„ Binding ditto	89	2	0			
„ Engraving and Lithography	1,515	19	6			
„ Soirée and Reception Expenses	101	2	7	141	14	7
„ Anniversary Expenses	40	12	0			
„ Coal, Lighting, &c.	163	1	5	1,274	13	2
„ Office Expenses	73	12	10			
„ House Expenses	563	2	0			
„ Tea Expenses	18	8	7			
„ Fire Insurance	55	5	0			
„ Taxes	51	11	11			
„ Law Charges	11	6	6			
„ Advertising Meetings	30	0	0			
„ Postage, Parcels, and Petty Charges	216	3	9			
„ Miscellaneous Expenses	92	1	2			
„ Carrington Donation				30	0	0
„ Water Research, Printing				2	16	6
„ Balance at Bankers Including £500 "Challenger" Account, £1,700 Catalogue Account, and £247 8*s.* 6*d.* Water Research Account.				2,780	7	1
„ Balance on hand, Catalogue Account	8	6	3	17	3	11
„ Ditto, Petty Cash	8	17	8			
				£10,025	2	10

Trust Funds.

	£	s.	d.	£	s.	d.
To Balance at Bank 12th November, 1893:—						
General Account	2,188	14	6	2,669	19	5
Fee Reduction Fund Account	143	6	4			
Scientific Relief Fund Account	290	16	10			
Joule Memorial Fund Account	97	1	9			
„ Scientific Relief Fund, Dividends, &c.	686	5	5	3,395	8	9
„ Donation Fund, Dividends, and Transfer from Jodrell Fund	387	6	2			
„ Rumford Fund, Dividends	62	1	9			
„ Bakerian and Copley Medal Fund, Dividends	54	3	6			
„ Keck Bequest, Dividends	23	5	6			
„ Wintringham Fund, Dividends	31	19	10			
„ Croonian Lecture Fund, Rent	50	7	6			
„ Davy Medal Fund, Dividends	31	19	2			
„ Gassiot Trust, Dividends and Bonds drawn	729	15	1			
„ Handley Fund, Dividends	186	2	4			
„ Jodrell Fund, Dividends	138	0	7			
„ Fee Reduction Fund, Dividends	435	19	7			
„ Darwin Medal Fund, Dividends, &c.	85	6	11			
„ Joule Memorial Fund, Dividends, &c.	149	15	5			
„ Brady Library Account, Interest	4	10	6			
„ Gunning Fund, Interest	60	0	0			
„ Buchanan Medal Fund	278	9	6			
				£6,065	8	2

	£	s.	d.	£	s.	d.
By Scientific Relief Fund, Grants	982	5	0	4,086	6	11
„ Donation Fund, Grants	665	0	0			
„ Rumford Fund	....	....	....			
„ Bakerian and Copley Medal Fund, Medal, Lecture, and Gift	58	12	7			
„ Keck Bequest, Payment to Foreign Secretary	27	9	1			
„ Wintringham Fund, Payment to Foundling Hospital	38	7	4			
„ Croonian Lecture Fund Payments	54	4	6			
„ Davy Medal Fund, Gold Medals	62	16	0			
„ Gassiot Trust, Payments to Kew Committee, and Purchase	756	1	2			
„ Handley Fund, Transfer to General Account, for Catalogue	186	2	4			
„ Jodrell Fund, Transfer to Donation Fund	138	0	7			
„ Fee Reduction Fund, Transfer to Royal Society General Account and Investment	573	1	0			
„ Darwin Medal Fund, Silver Medal and Gift	100	17	6			
„ Joule Memorial Fund, Investment	166	17	10			
„ Buchanan Medal Fund, Investment	276	12	0			
„ Balance at Bankers:—						
General Account	1,897	19	9	1,979	1	3
Fee Reduction Fund Account	6	4	11			
Joule Memorial Fund Account	79	19	4			
	1,984	4	0			
Less—Scientific Relief Fund Account—overdrawn	5	2	9			
				£6,065	8	2

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55A. 2R. 2P.), rent £85 per annum.

Ground Rent of House, No. 57, Basinghall Street, rent £380 per annum.

„ „ of 23 houses in Wharton Road, West Kensington, rents £253 per annum.

Fee Farm Rent, near Lewes, Sussex, £19 4*s*. per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, about £52 per annum, Croonian Lecture Fund.

Stevenson Bequest. Chancery Dividend. One-fourth annual interest on balance of Bequest still in Court.

£15,200 Mortgage Loan, 3½ per Cent., to the Duke of Norfolk.

£14,297 8*s*. 5*d*., 2¾ per Cent. Consolidated Stock, being £10,779 8*s*. 2*d*. on account of the following Funds:—

	£	*s*.	*d*.
Rumford Fund	2,330	0	0
Wintringham Fund	1,200	0	0
Gassiot Trust	400	0	0
Sir J. Copley Fund	1,666	13	4
Jodrell Fund	5,182	14	10

and £3,518 0*s*. 3*d*. in Chancery, arising from sale of the Coleman Street Estate.—General Purposes.

£913 0*s*. 3*d*. New 2¾ per cent. Stock { £403 9*s*. 8*d*. Bakerian and Copley Medal Fund.
£509 10*s*. 7*d*. Scientific Relief Fund.

£3,000 India 3½ per Cent. Stock.—General Purposes.

£1,300 India 3 per Cent. Stock.—General Purposes (Earl of Derby's Bequest).

£800 Midland Railway 3 per Cent. Debenture Stock.—Keck Bequest.

£370 3*s*. 7*d*. Midland Railway 4 per Cent. Perpetual Guaranteed Preference Stock.—General Purposes (Stevenson Bequest).

£5,660 Madras Railway Guaranteed 5 per Cent. Stock { General Purposes, £5,000.
Davy Medal Fund, £660.

£10,000 Italian Irrigation (Cavour Canal) Bonds.—The Gassiot Trust.

£9,061 6*s*. 8*d*. Great Northern Railway 3 per Cent. Debenture Stock { Scientific Relief Fund, £7,200.
The Trevelyan Bequest, £1,861 6*s*. 8*d*.

£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.—Donation Fund.

£2,725 „ „ „ 4 per Cent. Perpetual Preference Stock.—General Purposes (Stevenson Bequest).

£4,900 Metropolitan 3½ per Cent. Stock.—Fee Reduction Fund.

£258 9*s*. 2*d*. Metropolitan 3 per Cent. Stock—Buchanan Medal Fund.

£9,333 London and North Western Railway 3 per Cent. Perpetual Debenture Stock.—Fee Reduction Fund.

£20,908 " " " " 4 per Cent. Consolidated Guaranteed Stock.— { £6,000 Scientific Relief Fund.
£12,150 General Purposes.
£2,758 " " (Stevenson Bequest).

£5,000 " " " " Consolidated 4 per Cent. Preference Stock.—General Purposes.

£5,000 North Eastern Railway 4 per Cent. Preference Stock.—General Purposes.

£2,760 " " " Consolidated 4 per Cent. Guaranteed Stock.—General Purposes (Stevenson Bequest).

£2,200 South Eastern Railway 4 per Cent. Debenture Stock.—Darwin Medal Fund.

£4,340 South Eastern Railway 5 per Cent. Debenture Stock.—Scientific Relief Fund.

£3,333 London and South Western Railway 4 per Cent. Preference Stock.—General Purposes.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.—Handley Fund.

£1,000 London, Brighton, and South Coast Railway Consolidated Guaranteed 5 per Cent. Stock.—Joule Memorial Fund.

£4,000 Southern Mahratta Railway 4 per Cent. Debenture Stock.—General Purposes.

£311 19*s*. 0*d*. on Deposit Account at Bank.—Brady Library Account.

£50 on Deposit Account on behalf of the Committee.—Joule Memorial Fund.

£1,600 on Deposit Account at Bank, Mr. Ludwig Mond's Gift.—Catalogue Account.

£1,000 Policy in the Atlas Assurance Office, becoming due October 7th, 1899, No. 24644.—Catalogue Account.

£1,000 Bond.—Dr. Gunning.—Interest to be applied to the promotion of Physics and Biology.

JOHN EVANS, *Treasurer*.

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct.

A. AINSLIE COMMON.
WILLIAM A. TILDEN.
M. FOSTER.

We, the Auditors of the Treasurer's Accounts on the part of the Society, have examined these Accounts and found them correct.

D. E. HUGHES.
ALEX. B. W. KENNEDY.
ARTHUR W. RÜCKER.

Trust Funds. 1894.

Scientific Relief Fund.

£6,000 L. & N.W.R. 4 per Cent. Consolidated Guaranteed Stock.
£7,200 Great Northern Railway 3 per Cent. Debenture Stock.
£4,340 South Eastern Railway 5 per Cent. Debenture Stock.
£509 10*s*. 7*d*. 2¼ per Cent. Annuities.

Dr.	£ *s*. *d*.	£ *s*. *d*.		*Cr.* £ *s*. *d*.
To Balance { Income	862 16 5		By Grants	982 5 0
Less Capital over-invested	571 19 7			
		290 16 10		
" Dividends		665 0 7		
" Interest on Deposit		14 10		
" Donations (Capital)		20 10 0		
" Balance, Capital over-invested	551 9 7			
Less Income in hand	546 6 10			
		5 2 9		
		£982 5 0		£982 5 0

Donation Fund.

£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.
The Trevelyan Bequest. £1,861 6*s*. 8*d*. Great Northern Railway 3 per Cent. Debenture Stock.

	£ *s*. *d*.		£ *s*. *d*.
To Balance	641 19 2	By Grants	665 0 0
" Dividends	249 5 7	" Balance	364 5 4
" Transfer from Jodrell Fund	138 0 7		
	£1,029 5 4		£1,029 5 4

Rumford Fund.

£2,330 2¾ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	223	7	7	By Balance	285	9	4
„ Dividends	62	1	9				
	£285	9	4		£285	9	4

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift. £1,666 13*s.* 4*d.* 2¾ per Cent. Consolidated Stock.
£403 9*s.* 8*d.* New 2½ per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance	113	4	6	By Gold Medal	4	12	7
„ Dividends, New 2½ per Cent. Stock	9	15	4	„ Bakerian Lecture, Professor Thorpe	4	0	0
„ Dividend—Sir J. Copley's Fund	44	8	2	„ Gift:—Sir G. G. Stokes	50	0	0
				„ Balance	108	15	5
	£167	8	0		£167	8	0

The Keck Bequest.

£800 Midland Railway 3 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d
To Balance	4	3	7	By Payment to Foreign Secretary	27	9	1
„ Dividends	23	5	6				
	£27	9	1		£27	9	1

Wintringham Fund.

£1,200 2¾ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	38	7	4	By Payment to Foundling Hospital	38	7	4
„ Dividends	31	19	10	„ Balance	31	19	10
	£70	7	2		£70	7	2

Croonian Lecture Fund.

One-fifth of the clear rent of an Estate at Lambeth Hill, from the College of Physicians, about £52 per annum.

	£	s.	d.		£	s.	d.
To Balance	2	3	2	By Lecture (1894), Ramon y Cajal	50	0	0
„ Rent	50	7	6	„ Expenses	4	4	6
„ Balance	1	13	10				
	£54	4	6		£54	4	6

Davy Medal Fund.

£660 Madras Railway Guaranteed 5 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance	79	14	7	By Gold Medals	62	16	0
„ Dividends	31	19	2	„ Balance	48	17	9
	£111	13	9		£111	13	9

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.
£400 2¾ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	190	16	8	By Payments to Kew Committee	573	15	2
„ Dividends	495	0	9	„ Purchase of Ten Italian Irrigation Bonds	182	6	0
„ Ten Bonds drawn	234	14	4	„ Balance	164	10	7
	£920	11	9		£920	11	9

Handley Fund.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.

	£	s.	d.		£	s.	d.
To Dividends	186	2	4	By Transfer to Catalogue Account	186	2	4

The Jodrell Fund.

£5,182 14*s.* 10*d.* 2¾ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Dividends	138	0	7	By Transfer to Donation Fund	138	0	7

Fee Reduction Fund.

£4,900 Metropolitan 3½ per Cent. Stock.
£9,333 London and North Western Railway 3 per Cent. Perpetual Debenture Stock (Converted from £7,000 4 per cent. Stock).

	£	s.	d.		£	s.	d.
To Balance	143	6	4	By Transfer to Royal Society General Account	342	0	0
„ Dividends	435	19	7	„ Purchase of £200 Metropolitan 3½ per Cent. Stock	231	1	0
				„ Balance	6	4	11
	£579	5	11		£579	5	11

Darwin Medal Fund.

£2,200 South Eastern Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	477	9	5	By Silver Medal	0	17	6
„ Dividends	85	6	11	„ Gift: Sir J. D. Hooker	100	0	0
				„ Balance	461	18	10
	£562	16	4		£562	16	4

Joule Memorial Fund.

£1,000 London, Brighton, and South Coast Railway Consolidated Guaranteed 5 per Cent. Stock.
£50 on Deposit on behalf of the Committee.

	£	s.	d.		£	s.	d.
To Balance	97	1	9	By Purchase of £100 London, Brighton and South Coast Consolidated Guaranteed 5 per Cent. Stock	166	17	10
„ Interest on Deposit	1	6	7	„ Balance	79	19	4
„ Dividends	48	8	10				
„ Amount withdrawn from Deposit	100	0	0				
	£246	17	2		£246	17	2

Brady Library Fund.

£311 19*s.* on Deposit Account at Bank.

	£	s.	d.		£	s.	d.
To Amount on Deposit at Bank November 12th, 1893	307	8	6	By Balance on Deposit November 12th, 1894	311	19	0
„ Interest thereon	4	10	6				
	£311	19	0		£311	19	0

Gunning Fund.

£1,000 4 per Cent. Bond of His Excellency Dr. Gunning.

	£	s.	d.		£	s.	d.
To Balance	60	0	0	By Balance	120	0	0
„ Interest	60	0	0				
	£120	0	0		£120	0	0

Buchanan Medal Fund.

£258 9*s*. 2*d*. Metropolitan 3 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Cash, per Secretary of the Fund, Dr. Bristowe	276	12	0	By Purchase of £258 9*s*. 2*d*. Metropolitan 3 per Cent. Stock	276	12	0
„ Dividend	1	17	6	„ Balance	1	17	6
	£278	9	6		£278	9	6

Income and Expenditure Account from 12th November, 1893, to 12th November, 1894.

RECEIPTS.	£	s.	d.	£	s.	d.
To Balance at Bank and in hand 12th November, 1893 :—						
General Purposes	185	14	10			
Catalogue Account	2,200	0	0			
Challenger Account	500	0	0			
Water Research Account	0	5	0			
				2,885	19	10
„ Composition and Admission Fees				70	0	0
„ Annual Contributions				1,414	0	0
„ Rents				696	5	7
„ Dividends and Interest				2,596	10	7
„ Sale of Publications				678	0	1
„ Water Research Grants				250	0	0
„ Receipts on Account of Catalogue				234	18	10
„ Kew Committee Loan repaid				200	0	0
„ Proceeds of Sale of £722 14*s.* 1*d.* India 3 per cent. Stock				724	9	2
„ Proceeds of Sale of £270 11*s.* 11*d.* 2¾ per cent. Consolidated Stock				274	18	9
				£10,025	2	10

EXPENDITURE.	£	s.	d.	£	s.	d.
By Salaries, and Wages				1,443	10	4
„ Publications				3,259	2	2
„ Catalogue of Scientific Papers and Index :—						
Withdrawn from Deposit	500	0	0			
Paid out of General Funds	254	5	3			
				754	5	3
„ Books for the Library				321	9	10
„ Miscellaneous Expenses, Office, House, Receptions, &c.				1,446	7	9
„ Water Research Payments				2	16	6
„ Balance at Bank and in hand :—						
General Purposes	350	2	6			
Catalogue Account	1,700	0	0			
Challenger Account	500	0	0			
Water Research Account	247	8	6			
				2,797	11	0
				£10,025	2	10

The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	£3 yearly.	Total.
Nov. 30, 1893 ..	5	48	153	124	181	511
Since Elected ..		+3		+1	+15	+19
Since Compounded			+1		−1	
Since Deceased ..	−1	−3	−4	−8	−4	−20
Withdrawn or Removed ..				−2		−2
Nov. 30, 1894 ..	4	48	150	115	191	508

Account of Grants from the Donation Fund in 1893–94.

	£	*s.*	*d.*
Prof. J. Milne, in further aid of his Seismographic Investigations	100	0	0
Prof. Liveing, to enable Mr. W. J. Sell to continue certain Chemical Researches	25	0	0
W. T. Thiselton Dyer, to further aid Mr. Theodore Bent in making Botanical Collections in Southern Arabia ..	50	0	0
Measurement of Plants and Animals Committee (per F. Galton) for conducting Statistical Inquiries into the Measurable Characteristics of Plants and Animals....	50	0	0
Dr. H. Hicks, to enable Mr. J. Lewis Abbott to continue his Exploration of an Ossiferous Fissure near Ightham	10	0	0
Prof. G. H. Darwin, to assist him in his work on Periodic Orbits	30	0	0
Sir A. Geikie, to assist Miss M. M. Ogilvie in her Geological Researches	50	0	0
Prof. J. B. Sanderson, to assist Mr. H. M. Vernon in his Physiological Researches	25	0	0
Prof. McKendrick, to further aid Dr. Jack in Researches on Muscular Movements	5	0	0
Prof. W. C. Williamson, in aid of his Researches on Fossil Plants	10	0	0
Capt. Abney, for aid in his Researches on Colour......	40	0	0
Carried forward..........	£395	0	0

	£	s.	d.
Brought forward	395	0	0
Prof. W. M. Hicks, to aid him in his Experiments on the Higher Recalescence Point of Iron	20	0	0
Refunded Government Grant Committee (additions to Royal Society's telescope)	250	0	0
	£665	0	0

"Electrical Interference Phenomena somewhat analogous to Newton's Rings, but exhibited by Waves along Wires." By EDWIN H. BARTON, B.Sc., late "1851 Exhibition" Science Scholar. Communicated by Professor ARTHUR W. RÜCKER, M.A., F.R.S. Received February 20,—Read April 19,—Abbreviated* July, 1894.

INTRODUCTION.

The preliminary paper on this subject† gave the results of a single experiment, and approximately accounted for them by a mathematical theory of the reflexion and interference phenomena involved.

Since the publication of that paper the question of disturbances has been investigated, the experimental conditions improved, and various results obtained in confirmation of the original conclusions.

These matters form the subject of the present communication.

The apparatus employed for observing the interference phenomena is diagrammatically represented in fig. 1 and explained in the notes accompanying it.

In Experiment VII the electrometer was placed at HH′ instead of EE′, its usual position. In Experiments VIII and IX two electrometer needles were used, their respective attachments being at FF′ and HH′.

The lengths of SAM and MD, and the special construction of the abnormal part will be specified as required in the course of the paper when reference is made to the figure.

In the former paper‡ it was shown that in the case of a train of electrical waves advancing along the secondary from AA′ towards DD′ fig. 1, we have the following phenomena:—

(1.) A partial reflexion at BB′, the beginning of the abnormal part;

(2.) A partial reflexion at CC′, the end of the abnormal part; and

(3.) Interferences between the two sets of waves proceeding from BB′ and CC′ respectively.

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* The references given in the abstract of this paper ('Proceedings,' vol. 55, p. 350) are to the full manuscript, preserved in the archives of the Royal Society.

† 'Proc. Roy. Soc.,' vol. 54, pp. 85—96.

‡ *Loc. cit.*, p. 87.

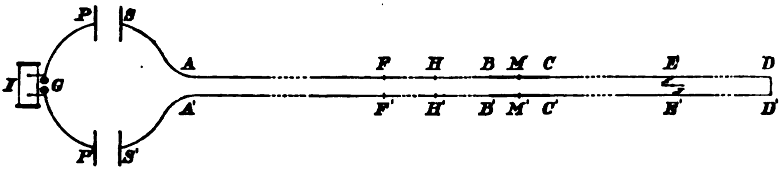

FIG. 1.—Diagram of Apparatus.

EXPLANATION OF FIG. 1.

I. Induction coil worked by two secondary cells. This coil could give regular sparks up to 2 cm. long; their frequency was about 25 per second.

G. Spark gap, usually 2 mm.

PGP′ was 204 cm. measured along the semicircle. The wires PG, GP′ were 2 mm. diameter.

PP′. Condenser plates of zinc 40 cm. diameter to form the ends of the Hertzian primary oscillator.

SS′. Similar plates at a distance of 30 cm. from P and P′, and forming the beginning of the long secondary which consists of copper wires about 1 mm. diameter. Those parts shown dotted in the figure were set up in the corridors adjoining the laboratory. With the above dimensions waves 9 mm. long were produced, their frequency being about 33 millions per second.

AA′ = BB′ = CC′ = DD′ = 8 cm. This distance was maintained throughout the secondary by pieces of dry wood at intervals of about 2 or 3 metres.

BCB′C′. The abnormal (or *altered*) part of the secondary used to produce the reflexion and interference phenomena.

MM′. The middle of the abnormal part.

EE′. The electrometer. The needle is uncharged: it therefore turns in the same direction whenever there is any potential difference between E and E′, whatever the sign of that difference. The needle was suspended by a quartz-fibre, and gave (though uncharged) a deflexion of about 35 scale divisions when the electrometer was connected to a battery of 10 Daniell's cells. In using the electrometer for the electric waves first throws, not steady deflexions, were always read.

DD′. Wire bridge across the main wires.

FH = HB = ED = a quarter of a wave-length.

Hence, as the length of the abnormal part is gradually increased, the total energy of the reflected waves is thereby periodically increased and decreased. It was thus seen that the length of the abnormal part of the secondary corresponds to the thickness of the air film in the optical phenomena known as Newton's Rings, and that the reflexions of the electrical waves at the beginning and end of the abnormal part correspond respectively to the reflexions of light at the first and second faces of the air film.

For the experiments now to be described the results of the mathematical theory previously developed* are as follows:—

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* *Loc. cit.*, equations (13), p. 90, and equations (18), p. 92.

(1.) For the *single reflexion* at the beginning of the abnormal part.

Let the amplitude of the original wave-train be a, and that of the reflected one be ab, then

$$b = -\frac{r-1}{r+1} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1),$$

where r is the ratio of the electrostatic capacity of unit length of the abnormal part of the secondary to that of its normal parts.

(2.) For the *interference* of the wave-trains proceeding from the beginning and end of the abnormal part of the secondary.

Let the incident wave-train be

$$\phi = ae^{-\alpha t+\alpha_1 x} \sin(\beta t - \beta_1 x) \quad \ldots\ldots\ldots\ldots\ldots \quad (2),$$

where ϕ denotes electrostatic potential, $\alpha/\alpha_1 = \beta/\beta_1 = v_1$, the velocity of propagation of the waves, and a, α and β are constants depending respectively upon the amplitude, the primary damping and the frequency.

Let the energy of the wave-train, when incident upon the beginning of the abnormal part (BB′ fig. 1) be ι, let $\iota\tau$ be the total energy of all waves transmitted through the abnormal part, and let $\iota\rho$ be that of all the waves returned along BA after one or more reflexions at C. Then

$$\left.\begin{aligned} \tau &= \frac{1-b^2}{1+b^2} + \mathrm{U}\,; \\ \rho &= \frac{2b^2}{1+b^2} - \mathrm{U}\,; \\ \text{where} \quad \mathrm{U} &= \frac{1-b^2}{1+b^2} \cdot 2b^2 e^{-\alpha t_2} \frac{(\alpha/\beta)\sin\beta t_2 + \cos\beta t_2 - b^2 e^{-\alpha t_2}}{1 - 2b^2 e^{-\alpha t} \cos\beta t_2 + b^4 e^{-2\alpha t_2}} \end{aligned}\right\} (3).$$

and t_2 denotes the time occupied by the waves in twice traversing the length, BC, of the abnormal part of the secondary.

APPROXIMATE THEORY OF DISTURBANCES.

Thus far, then, we have dealt only with a *single* incidence of the wave-train upon the abnormal part of the secondary, and have found expressions for the two trains thereby produced. But in order to represent more completely what occurs with the apparatus arranged as in fig. 1, it is further necessary to trace the history of the two wave-trains to which the original one thus gives rise.

If the reflected wave-train were quickly extinguished and never again reached the plates, SS′ fig 1, and if the transmitted wave-train after passing the electrometer, and thus giving the deflexion by which it is measured, were also quickly extinguished without again reaching the abnormal part CB, then, and then only, would the theory hitherto developed suffice. Clearly, however, these conditions are not fulfilled.

On the contrary, the waves go to and fro along the secondary, suffering—

(α) Total reflexion with reversal of electrification at the short circuited end DD′ (fig. 1),

(β) Total reflexion without reversal of electrification at SS′, and

(γ) Partial reflexion and partial transmission at each incidence on the abnormal part BC.

We have, thus, a case of binary fission at each incidence of a train on the abnormal part, each such fractional train returning to the abnormal part to be further, in like manner, subdivided. Theoretically, this process continues *ad infinitum.*

From these considerations it may easily be inferred that, to avoid hopeless confusion, the distance from the abnormal part to either end of the secondary must exceed half the length of the train of electrical waves, or in symbols :—

$$\mathrm{SB} > \tfrac{1}{2}\mathrm{X} \text{ and } \mathrm{CE} > \tfrac{1}{2}\mathrm{X} \ldots\ldots\ldots\ldots\ldots \quad (\mathrm{A}),$$

where X is the effective length of the wave-train, and the other letters refer to fig. 1.

It was also found necessary to avoid placing the abnormal part midway between the two ends, S and D, of the secondary. For, in that case, the two sets of waves respectively reflected at and transmitted through the abnormal part would, after travelling to opposite ends of the secondary, again meet at the middle and interfere with each other. This disturbance was sufficiently obviated by fulfilling the condition—

$$(\mathrm{SM} \sim \mathrm{MD}) > \tfrac{1}{2}\mathrm{X} \ldots\ldots\ldots\ldots\ldots\ldots \quad (\mathrm{B}).$$

But when conditions (A) and (B) are both fulfilled, there is still a residual disturbance. For, although the electrometer is placed to receive the systems transmitted through the abnormal part, it, in consequence of their repeating coursings to and fro, actually receives also feebler systems of reflected waves.

Thus, let the fraction of incident wave-energy transmitted by the abnormal part be τ, and let the ratio of the electrometer readings with and without the abnormal part be τ', then the author has shown that τ' is approximately given by the equation

$$\tau' = \tau \frac{1-s_1 s_2}{(1-s_1)(1-s_2)+\tau(s_1+s_2-2s_1 s_2)} \ldots\ldots\ldots \quad (4).$$

where s_1 and s_2 express the attenuation of energy suffered by the waves in passing along lengths of the secondary equal respectively to twice SM and twice MD (fig. 1).

τ' is always a little greater than τ except in the limiting cases where $\tau = 0$ and $\tau = 1$. Thus, the experimentally-determined ratio needs a negative correction. This is easily applied by graphical methods.

Now, since τ' is a function both of τ and of the attenuation (or *secondary* damping) it becomes necessary, in order to utilise Equation (4), to estimate the value of this damping.

The theory of one method devised for this purpose is as follows:—

Suppose the arrangement of apparatus shown in fig. 1 to be modified thus. Let the 1-mm. diameter copper wires be continued beyond DD′ for some distance, and after that let the wires for a further length be of iron of 0·1 mm. diameter. Also let the bridge shown at DD′ be movable and capable of being placed at pleasure anywhere beyond the electrometer.

And consider, first, the changes in the electrometer throws as the bridge is moved from the electrometer, but still always upon the copper wires. Let the electrometer throws be plotted as ordinates, and the distances of the bridge from the electrometer as abscissæ. Then, for positions of the bridge immediately beyond the electrometer the curve so obtained is conspicuously wavy, and continues sensibly so for a distance equal to half the appreciable length of the wave-train. This is the part corresponding to the curve shown by V. Bjerknes ('Wied. Ann.,' vol. 44, p. 522, 1891), and is due to the interferences between the wave-trains advancing towards and reflected from the bridge DD′. Beyond this part, whatever the rate of decay of the waves, we have a continuous droop in the curve. The exact form of the curve depends, in part, upon this decay, and the ordinate of the asymptote to the curve may be shown to be a simple function of σ (where $e^{-\sigma x}$ is the law of decay of the amplitude of the waves).

Let the distance SE (fig. 1) be L, and let y_x be the ordinate of the curve at the point whose abscissa is x, the ordinate at the origin being unity. Then we have for the equation of the curve—

$$y_x = \frac{1-e^{-4\sigma L}}{2} \cdot \frac{1+e^{-4\sigma x}}{1-e^{-4\sigma(L+x)}} \quad \ldots\ldots\ldots\ldots\ (5).$$

and for that of its asymptote—

$$y_\infty = \tfrac{1}{2}(1-e^{-4\sigma L}) \quad \ldots\ldots\ldots\ldots\ldots\ldots\ (6).$$

The ratio y_∞ is difficult to determine if the copper wires only are used, but with the thin iron wires the electrometer throws rapidly fall off to their minimum value, and thus y_∞ is readily obtained as desired.

EXPERIMENTS.

The chief experiments made will now be dealt with in the following order:—

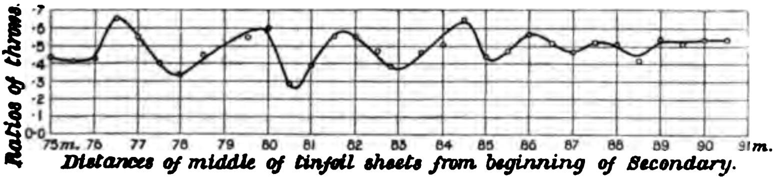
(1.) Undesired interferences of separate wave-trains. Experiments I—III.
(2.) Estimate of rate of decay of waves. Experiment IV.
(3.) Analogy to Newton's rings by transmission under improved conditions. Experiments V and VI.
(4) Analogy to Newton's rings by reflexion. Experiment VII.
(5) Abnormal parts which produce no reflexion. Experiments VIII and IX.

Experiment I.—Undesired Interferences of Distinct Wave-Trains simultaneously reflected at and transmitted through the Abnormal Part of the Secondary.

To investigate this question the arrangement of apparatus shown in fig. 1 was adopted. The abnormal part consisted of a single pair of tinfoil sheets each 50 cm. long and 32 cm. deep, the two being hung immediately opposite each other, one on each wire of the secondary. The total length, SAD, of the secondary was 162 m., the distance SAM was varied from 75 m. to 90·5 m. by steps of 0·5 m. each. Electrometer readings with and without the sheets were taken alternately in order to eliminate the possible errors due to irregular working of the primary sparks. The observations taken are shown in Table I.

The result is graphically exhibited in the curve, fig. 2.

FIG. 2.—Curve showing Undesired Interferences.



Experiment II.—Interferences as in Experiment I, but with Thin Iron Wires at the end of the Secondary.

Various experiments were made with the secondary lengthened about 70 mm. by the addition of thin iron wires, in the hope that they would absorb all the incident waves and thus do away with their repeated coursings and consequent interferences. The result was an

Table I.

Electrometer throws.			Distances SAM, fig. 1.	Ratios of throws, *i.e.*, quotients of cols. 3 and 2.
Without abnormal part.		With abnormal part.		
Actual observations.	Interpolated means.			
27·7	25·8	11·2	75 m.	0·43
23·8	26·8	11·1	75·5	0·41
29·7	27·6	12·0	76	0·43
25·6	26·0	17·1	76·5	0·66
26·4	29·2	16·0	77	0·55
32·0	32·2	12·8	77·5	0·40
32·5	32·2	10·8	78	0·34
32·0	31·8	14·4	78·5	0·45
31·6	28·8	15·8	79·5	0·55
26·1	27·7	16·6	80	0·60
29·4	31·5	9·0	80·5	0·29
33·6	33·6	13·2	81	0·39
33·6	34·4	19·0	81·5	0·55
35·2	35·0	19·4	82	0·55
34·7	34·0	16·3	82·5	0·48
33·1	32·7	12·8	82·8	0·39
32·3	33·6	15·6	83·5	0·46
35·0	33·4	18·0	84	0·51
31·8	31·4	20·5	84·5	0·65
31·0	32·5	14·3	85	0·44
34·0	33·1	15·7	85·5	0·47
32·2	33·1	18·4	86	0·56
34·0	33·4	17·3	86·5	0·52
32·8	31·1	14·8	87	0·47
29·4				
32·5	30·6	15·9	87·5	0·52
28·7	27·2	13·9	88	0·51
25·7	20·6	8·7	88·5	0·42
15·5				
30·3	30·4	16·2	89	0·53
30·6	29·6	15·1	89·5	0·51
28·6	27·4	14·5	90	0·53
26·3	25·8	13·7	90·5	0·53
25·3				

undulating curve whose ordinates lay between the limits 0·51 and 0·36. Thus the extreme values of the ordinates differed from their mean value by about 17½ per cent. of the latter. The electrometer throws, however, were now reduced to about one-third of their former value. This was owing to the continuation of the wires so far beyond the electrometer instead of their termination by a bridge distant only a quarter wave-length from it. It seemed, therefore, that the iron wires did more harm than good, and they were accordingly abandoned in favour of the arrangement described in the next experiment.

Experiment III.—Search for Interferences as in Experiment I, but with the Abnormal Part away from the middle of the Secondary.

As the outcome of Experiment II the theory resulting in condition (B), p. 71, was developed, and the present experiment tried in accordance therewith. The effective length of the wave-train was experimentally found to be about 70 m. Hence, to fulfil (B) it was necessary to make $(\mathrm{SAM} \backsim \mathrm{MD}) > 35$ m.

The apparatus shown in fig. 1 was then arranged thus: SAD = 164 m. SAM varied from 95·25 m. to 105·75 m., and electrometer throws were taken alternately with and without a pair of tinfoil sheets as in Experiments I and II.

This experiment was performed twice and the mean result, when plotted as in Experiment I, gave a curve undulating between the limiting ordinates 0·55 and 0·46. Thus the extreme values of the ordinates differed from their mean value by about 9 per cent. of the latter.

It is thus seen that, although the straight line hoped for was not obtained, yet this curve is less wavy than either of the two previous ones. And since, also, this arrangement yields the maximum effect at the electrometer, it was adopted in the experiment for the analogy to Newton's rings by transmission hereafter described.

Experiment IV.—Estimate of the Rate of Decay of the Waves in their Advance along the Secondary.

The arrangement of the apparatus adopted for this determination has already been described under the headings of Theory (p. 72). In this experiment the droop of the curve sought was expected to be very slight. Consequently, for the sake of accuracy, each point of the curve was determined by twenty-one electrometer readings. Six sets of observations were made with SAE, fig. 1, = 160 m. They yielded a curve whose asymptote was—

$y_\infty = 0{\cdot}34$; whence we obtain

$$\sigma = 0{\cdot}0018 \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (7).$$

A second series of observations with SAE = 91·5 m. afforded the values $y_\infty = 0{\cdot}31$ and

$$\sigma = 0{\cdot}0029 \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (8).$$

Experiment V.—Analogy to Newton's Rings by Transmission under best Conditions.

This experiment was made with exceptional care, as it is the chief one of the series. Most of the others either served to indicate the conditions adopted in this one or were in other ways subsidiary to it. The construction of the abnormal part was such as to reflect a large fraction of the wave-energy incident upon it. Its position was so chosen as to avoid the interferences of higher order already noticed. Thus the true effect sought was large, the disturbing ones small. The arrangement of the apparatus is shown in fig. 1. The details are as follows.

The abnormal part consisted of tinfoil sheets each 32 cm. deep and 50 cm. long. These were hung opposite each other upon the two wires of the secondary. Lest a shift of the middle point of the sheets along the wires might have a slight disturbing effect on the electrometer throws, the abnormal part was always lengthened or shortened by the same amount at *each end*, thus leaving the middle point M undisturbed. The length SAM was throughout the experiment 101 m., MD being 63 m.* so as to comply with condition (B). The tinfoil sheets were kept properly spaced by very accurately cut wood separators, one for each half metre's length of the sheets. The same separators, but at longer intervals, were used throughout the line. The vertical edges of consecutive sheets were made to overlap about 2 cm. and allowed to hang in simple contact.

Lest this contact should be insufficient and so involve an error, the sheets were built up in this way to a length of 2·25 m. (a quarter of a wave-length), and the ratio determined of the wave-energy transmitted by it to that incident upon it. Then an abnormal part, consisting of zinc plates soldered together was substituted, and the fraction of wave-energy transmitted again determined. In each case, to insure accuracy, twenty-one electrometer readings were taken alternately with and without the abnormal part. The two ratios thus determined were:—†

(1) For the tinfoil sheets	0·135 ± 0·007
(2) For the zinc plates soldered	0·135 ± 0·002

Hence this apparently imperfect contact seemed sufficient for the case, and was adopted in the main experiments now under consideration. A spark gap of 2 mm. was used throughout both in the experiments just described and in what now follows.

The method of taking the observations so as to eliminate the possible errors due to the irregularities in the sparking of the primary was as follows. Several electrometer throws were taken, at first without any tinfoil sheets on the wires, then a couple of readings with, say, two sheets on each wire; then two readings with four sheets on each wire, and so forth. After the abnormal part had thus been built up to a considerable length, it was then in like manner shortened, two readings being taken for each length, concluding with several readings without any sheets at all on the wires. The whole process was carried out four times, the lengths used in the various cases being varied so as to obtain eight electrometer readings for each half metre's length of the abnormal part. Thus, in the first and third sets of observations, the lengths of the abnormal part were approximately 0, 1 m., 2 m., 3 m. .... 3 m., 2 m., 1 m., 0; whereas, in the second and fourth sets, the lengths were about 0, 0·5 m., 1·5 m., 2·5 m. .... 2·5 m., 1·5 m., 0·5 m., 0.

---

* When making the corresponding experiment, described in the former paper p. 96, SAM was taken equal to MD. I had not, up to then, seen that it was desirable to avoid that arrangement.

† The probable errors were calculated by the formulæ given in Kohlrausch's 'Leitfaden der Praktischen Physik,' Leipzig, 1892, pp. 1—3, and proved in Dr. B. Weinstein's 'Handbuch der Physikalischen Maassbestimmungen,' vol. i.

A single set of readings up to nearly 7 m. length of abnormal part is given in Table II. During the process of lengthening the abnormal part the ratios are obtained by taking for the divisor the *initial* mean throw without abnormal part, whereas, during the shortening, the like *final* value is used as the divisor. Hence the double values for the ratio corresponding to the length 6·72 m. The last column of the table contains the mean of the ratios during lengthening and shortening.

The kind of agreement obtained by the four sets of observations is shown by the two curves in fig. 3. In these curves the abscissæ represent the lengths of the abnormal part. In the upper curve the ordinates represent the lengths of the abnormal part. In the upper curve the ordinates represent the mean ratios of the throws obtained in the first and third sets of observations with and without the abnormal part. The ordinates of the lower curve give the values of the same ratios, as obtained from the second and fourth sets of readings.

FIG. 3.—Preliminary Curves from Experiment V.

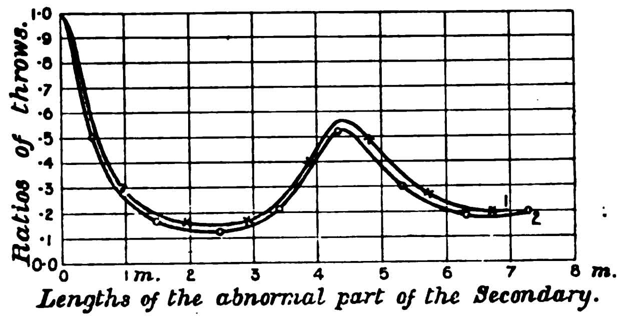
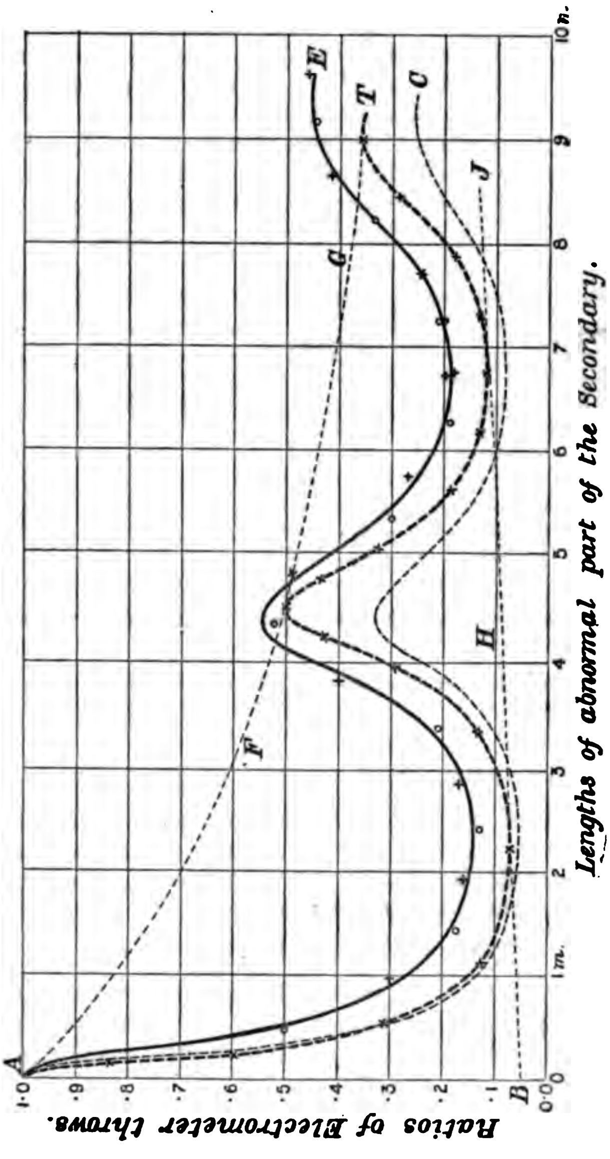


Table II.

Electrometer throws.	Mean throws.	Length of the abnormal part.	Ratio of throws with & without it.	Mean ratios.
40·8 51·8 36·2 46·2 39·2	42·8	m. 0·00	1·00	1·00
13·9 12·6	13·2	0·97	0·31	0·30
5·7 5·8	5·8	1·93	0·135	0·14
5·8 6·2	6·0	2·89	0·14	0·145
16·9 17·9	17·4	3·85	0·41	0·39
17·9 17·9	17·9	4·80	0·42	0·42
9·6 9·5	9·6	5·76	0·224	0·23
6·7 6·6	6·6	6·72	{0·154 0·174}	0·164
9·0 8·9	9·0	5·76	0·24	0·23
16·2 15·9	16·0	4·80	0·42	0·42
14·0 14·2	14·1	3·85	0·37	0·39
5·6 5·6	5·6	2·89	0·15	0·145
5·7 5·9	5·8	1·93	0·15	0·14
11·8 10·3	11·0	0·97	0·29	0·30
38·4 34·3 40·7 36·2 40·4	38·0	0·00	1·00	1·00

The process was afterwards continued from this point onwards to a length of 9·5 m. The final mean result of the entire experiment is shown in the curve E, fig. 4.

FIG. 4.—Analogy to Newton's Rings by Transmission.



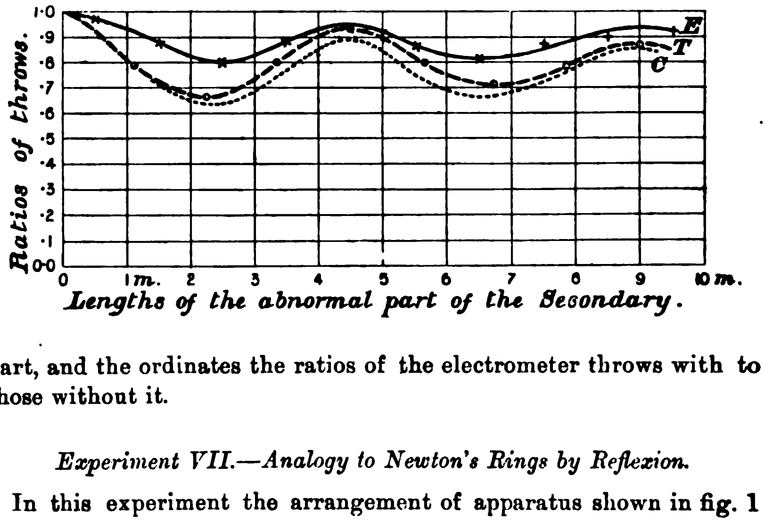
Experiment VI.—Analogy to Newton's Rings, as in Experiment V, but with different Abnormal Part.

In this experiment the lengths of SAD and the position of M were precisely as in Experiment V. The abnormal part, however, consisted simply of the ordinary wires, but put closer together there than elsewhere, namely, 0·68 cm. apart instead of 8 cm.

The electrometer readings were taken thus:—First, with no abnormal part; next, with one of 0·5 m. long; again with no abnormal part, then with one 1·5 m. long, and so forth. The readings with and without the abnormal part were always alternated, and the ratios of the throws taken as in Experiments I—III.

The result of this set of observations is graphically exhibited in the curve E, fig. 5. The abscissæ represent the lengths of the abnormal part, and the ordinates the ratios of the electrometer throws with to those without it.

FIG. 5.—Second Case of Analogy to Newton's Rings by Transmission.



Experiment VII.—Analogy to Newton's Rings by Reflexion.

In this experiment the arrangement of apparatus shown in fig. 1 was modified by removing the electrometer from EE′ and inserting it at HH′, where HB is a quarter of a wave's length. Throughout the experiment the lengths were as follows:—

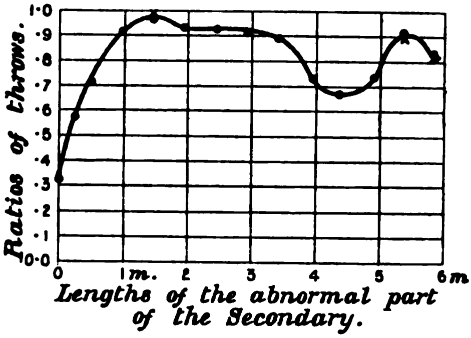
SAD = 235 mm., SAB = 164 mm.

The abnormal part was of tinfoil sheets, 32 cm. deep and 50 cm. long, precisely as in Experiment V, while the end BB′ remained fixed, the length BC was varied from nothing to 6 m., advancing by steps of half a metre.

The electrometer throws were taken, first with a bridge at BB′, then without the bridge but with an abnormal part consisting of a single pair of tinfoil sheets, next with the bridge at BB′, then again without the bridge but with an abnormal part of two pairs of sheets, and so forth, the readings with and without bridge being always alternated.

The result is plotted in the curve shown in fig. 6, the lengths of the abnormal part being taken as abscissæ, and the ratios of electrometer throws as ordinates.

Fig. 6.—Analogy to Newton's Rings by Reflexion.



On this curve two apparently anomalous humps may be noticed in the neighbourhood of 1·5 m. and 5·5 m. lengths of the abnormal part. These, however, are not due to errors of observation; for, on carefully repeating the experiment in these regions the first results were confirmed, as shown by the double dots made at those places.

A few readings were also taken with the above general arrangement, but an abnormal part, consisting of the ordinary wires nearer together, just as in Experiment VI. It was thus found that the electrometer throws were about three times as great with an abnormal part a quarter-wave long as with one a half-wave long.

Experiment VIII.—An Abnormal Part which Produces no Reflexion.

Having thus far experimented with the desired interference phenomena, and discussed the various disturbances involved, it now seems of interest to notice particular forms of the abnormal part which reflect no portion of the wave-energy incident upon them. This and the following experiment were tried as tests of the truth of the theory for a single reflexion.

However, to establish the entire absence of reflexion would be very difficult with the single-needle electrometer previously used, because the slightest irregularity of the primary sparking might be mistaken for the effect of reflexion. And no repetition of observations, however extended, would justify the conclusion that the energies transmitted with and without the abnormal part were absolutely equal. It became expedient, therefore, to use an electrometer which gave deflexions *only* when reflected waves were present. Such an instrument was, fortunately, ready to hand in the differential electrometer used and described by Dr. von Geitler,* and kindly left by him for my use.

In this form of electrometer two needles, rigidly connected with each other and hanging upon the same quartz fibre, are employed. One of these experiences a right-handed torque, owing to attachments at one pair of points on the secondary, whenever these points have a potential difference of either sign, while the second needle, under like conditions, experiences a left-handed torque, owing to attachments at a second pair of points on the secondary, and distant a quarter of a wave's length from the first pair. Thus, when the instrument is properly adjusted, the *mere passing* of a wave-train leaves the needles undisturbed.

If, however, reflexion is by any means produced near the electrometer, so that it is in a region of *standing* waves, it may give a throw.

The arrangement of apparatus adopted may be seen from inspection of fig. 1, which was modified by the removal of the single-needle electrometer from EE′, and the insertion of the differential electrometer at FF′ HH′. The first needle was influenced by attachments to F and F′, and the second by attachments to H and H′. The main wires between the first and second pairs of points were led in a loop, so that they first passed the electrometer at FF′, and a second time at HH′. Thus, all the attachments between the main wires and the electrometer were quite short.

The wires in the normal part of the secondary were 0·116 cm. diameter. The abnormal part consisted also of copper wires, and were in this experiment 0·019 cm. diameter, and 2·25 m. long.

These were tried at two distances apart, namely, 1 cm. and 1·5 cm. The electrometer throws in these cases were $-1·8$ and $+0·6$ scale divisions respectively. Whence, by interpolation, we have 1·37 cm. nearly, as the distance apart at which these wires would yield no reflexion. The electrometer throw with a bridge at BB′ was twenty-four scale divisions.

Experiment IX.—A Second Example of no Reflexion from an Abnormal Part.

In this experiment everything was the same as in the preceding one, except that the abnormal part consisted of wires thicker and wider apart than the rest of the line, instead of being thinner and nearer together. The wires in question were 0·55 cm. diameter, and, when placed at 66 cm. apart, there was practically no reflexion.

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* Wiedemann's 'Annalen,' vol. 49, pp. 188—189, 1893.

COMPARISON OF THEORY AND EXPERIMENT.

It now becomes of interest to compare the results of Experiments V to IX with the theories advanced concerning them.

In Experiment V we have the capacity per unit length of the normal part in electrostatic units approximately given by $1/(4 \log_e d/w)$, where d is the distance between the centres of the wires and w their radius. For the abnormal part, without any correction for the edges, we have capacity per unit length equals $D/4\pi d$, where D is the width of the tinfoil sheets. Now d was 8 cm., w was 0·05 cm. nearly, and D 32 cm. Thus we obtain for r, the ratio of the capacities, 6·5 nearly. If, however, in estimating the capacity of the abnormal part, a correction for the edges is made, we obtain values of r ranging up to 8 or 9, according to the length of the abnormal part under consideration, and the formula used for the correction.

Taking as a typical case the length of abnormal part to be 2·25 m., on which special experiments were made, and correcting for the edges by the approximate formula given by Professor Kohlrausch* we obtain $r = 9$, nearly. Whence, from equation (1), $b = -0·8$.

It will be seen from the equations (3) that it is further necessary to know the constants α and β. These were determined by experiments similar to those previously devised and carried out by V. Bjerknes.†

In my case, however, the electrometer readings were alternated with the bridge at the variable distance x, and at the quarter wave-length distance beyond the electrometer. A spark gap of 2 mm. was used throughout.

Thus were obtained for the electrical waves advancing along the wires, the wave-length $\lambda_1 = 9$ m.; and for the constant involving the primary damping, we have $\gamma_1 = 2\pi\alpha/\beta = 0·524$, or say $\gamma_1 = 0·5$ nearly.

Then, assuming that the velocity of propagation of the waves along the wires is practically that of light in air,‡ we obtain, from

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* 'Leitfaden der praktischen Physik,' Leipzig, 1892, p. 357.

† Wiedemann's 'Annalen,' vol. 44, pp. 519—522, 1891.

‡ This is known to be the case from various experiments, and also from theoretical considerations. See, for example:—

(1.) Professor Hertz, "Ausbreitung der elektrischen Kraft," Leipzig, 1892, or the English translation by Professor D. E. Jones, B.Sc.

(2.) MM. Sarasin and De la Rive, 'Archives des Sciences Physiques et Naturelles,' vol. 29, No, 5. Genève. 1893.

(3.) Professor J. J. Thomson, F.R.S., "Recent Researches on Electricity and Magnetism," pp. 279 and 451—467.

(4.) Professor Oliver Lodge, F.R.S., 'Phil. Mag.,' August 1888, vol. 26, p. 228; 'Report Brit. Assoc.,' 1888, p. 567; and 'Proc. Roy. Soc.,' vol. 50, pp. 29—39.

$\lambda_1 = 9$ m., the value of the frequency, namely, $\beta/2\pi = 33\frac{1}{3}$ millions per second, nearly.

From these data the curve T, fig. 4, is plotted, the lengths of the abnormal part being abscissæ and the corresponding fractions of incident wave energy transmitted being ordinates.

It is thus noticeable that the theoretical curve, though of similar form, lies wholly below the experimental one. If, however, we apply to the latter, a correction based upon Equation (4) and the results of Experiment IV, we obtain curves which lie wholly below the theoretical one. One such is shown in curve C, fig. 4, in which σ is taken equal to 0·0029.

Thus, while the comparison of theory and experiment has not resulted in the establishment of an exact agreement between them, it is yet so far satisfactory to notice* that the three curves are precisely similar in form, and that the theoretical one lies entirely between the two experimental ones, with and without correction. It is also noteworthy that Mr. Yule, in his recent work† on "The Passage of Electrical Wave-trains through Layers of Electrolyte," found a discrepancy of the same sign and of like amount between the experimental curve and the theoretical one.

The dotted lines above and below the main curve in fig. 4 indicate the limits between which the latter lies. These limiting lines would be horizontal but for the primary damping of the waves emitted by the oscillator.

We thus see that the curve in the neighbourhood of $\lambda_1/4$ affords the best means of determining b when it is nearly unity. For the troughs of the curve are broad, and therefore the curve is at that part for some distance nearly parallel to the axis of the abscissæ. Thus a great deviation from the true length $\lambda_1/4$ would make but a small one in the corresponding ordinate. Secondly, for large values of b, this ordinate is but very slightly affected by the primary damping. Hence a great error in the measurement of a makes but a slight one in the determination of b from this part of the curve. Thus, taking $\gamma_1 = 0{\cdot}5$, and accepting the experimental curve, we obtain $b = -0{\cdot}71$ and $r = 6$, or taking the experimental curve, with correction, as shown by the lower line in fig. 4, we get $b = -0{\cdot}8$ and $r = 9$. And it was between these limits 6 and 9 that r was calculated to lie.

In Experiment VI.—Since the same wires were used for the abnormal part but closer together, namely, at a distance $d' = 0{\cdot}68$ cm. instead of $d = 8$ cm., we have for the ratio of capacities

$$r = \frac{1/4 \log (d'/w)}{1/4 \log (d/w)} = \frac{\log (8/0{\cdot}05)}{\log (0{\cdot}68/0{\cdot}05)} = 2.$$

---

* Professor Hertz considered the agreement between experiment and theory as close as was to be expected.

† 'Phil. Mag.,' pp. 543—544, Dec. 1893.

Thus, by Equation (1) we obtain $b = -\frac{1}{3}$. From this value of b and those last used for α and β we derive the curve shown by T, fig. 5.

This, as in the case of Experiment V, lies wholly below the experimental curve. But here, again, the correction to the experimental curve applied in accordance with Equation (4), and the higher value of σ from Experiment IV yields the curve C, fig. 5, lying still lower than the theoretical one.

In Experiment VII.—It may be seen from the Equations (3) or from general considerations that the total intensity of the reflected waves must be complementary to that of the transmitted ones, that is $\rho + \tau = 1$. Consequently the theoretical curve showing ρ as ordinates instead of τ would be obtained by inverting the theoretical one for τ. Hence, we see that the general characteristics of the curve for ρ may be thus outlined:—

(1) A damped wavy-formed curve with—
(2) *narrow* troughs at $l = 0, \lambda/2, \lambda$, &c., and
(3) *broad* crests at $l = \lambda/4, 3\lambda/4$, &c.,

where l is the length of the abnormal part. These general characteristics are possessed by the experimental curve obtained. It must, of course, be borne in mind that the ordinates of the curve thus experimentally determined do not represent ρ, but represent approximately ratios proportional to $1 + \rho$, since we have both the reflected and the *incident* waves passing the electrometer. On this account the wavy form of this curve is less strongly marked than in the case of transmission. Probably also other, and undesired, interferences arose between the on-coming and reflected waves in the neighbourhood of the electrometer, thus causing the two anomalous humps at 1·5 m. and 5·5 m.

In Experiment VIII.—For the cases to which Equation (1) applies we see that there is no reflexion when $r = 1$, that is, when no change in the capacity of the secondary occurs. Now the approximate expression for the capacity of two equal parallel cylinders depends not upon their *absolute* but upon their *relative* dimensions only. Hence, if Equation (1) is correct, it must be possible to arrange a part of the wires in a form which appears very abnormal, but yet so as to produce no reflexion. We have simply to introduce, at any part, thinner wires placed proportionately nearer together or thicker ones in like manner further apart. Thus, since in either case, the capacity is unaltered by the change in question, we have $r = 1$, and the fraction of wave-energy reflected disappears.

In the experiment under consideration the readings taken pointed to the distance 1·37 cm., from centre to centre of wires, as being that which, with the wires in use, would give no reflexion. Theory gives as the correct distance 1·32 cm. Thus the discrepancy is not great;

in fact, it may perhaps be within the limits of the errors of observation.

In Experiment IX.—Here theory points to a distance apart of 38 cm. as that which should produce no reflexion. The experiment, however, gave 66 cm. This discrepancy at first sight appears serious. When, however, it is noticed that in the case of cylinders widely separated a very great further increase in their distance is required to produce a small decrease in their capacity, the discrepancy does not seem so great. Indeed, when the distance between the wires in this case is altered from 38 cm. to 66 cm., the capacity is only changed by about 10 per cent. It is conceivable that a discrepancy of that order might be due to the sloping portions of wire which served to connect the normal wires, spaced at 8 cm., with the abnormal ones spaced at 66 cm.

SUMMARY OF CHIEF RESULTS.

The principal conclusions to be drawn from the foregoing theory and experiments taken in conjunction may be stated as follows:—

(1.) In experimenting with electrical waves of high frequency passing along a pair of parallel wires with short-circuited end, and containing a portion which produces partial reflexion, it is necessary to make right choice of the lengths before and after such source of reflexion in order to avoid disturbing interferences.

(2.) A sudden change in the capacity of the secondary produces a partial reflection at that place of change. The ratio of the amplitudes of the reflected and incident wave-trains may be expressed as a simple function of the change in capacity. See Equations (12) and (13) in the previous paper.

(3.) If a sudden change in the capacity of the secondary is succeeded by a sudden reversion to the normal state of the wires, then reflexions occur at each of these places of abrupt change. And, if the distance between these two points is comparable with a wave-length, then the waves proceeding from them will interfere, consequently when the distance in question is increased a series of maxima and minima successively obtain, essentially analogous to those which simultaneously occur in the optical phenomena known as Newton's Rings.

(4.) If the secondary has a part which, though abnormal in appearance, introduces no change in its capacity, then no reflexion is produced by it.

For each of the above statements theoretical grounds and experimental confirmation have been adduced. And, although the two are not in exact quantitative agreement, yet I think it will be admitted that they are approximately so, and that their accordance in all general respects is such that they support each other and warrant the conclusions drawn from them.

I have again to acknowledge my deep indebtedness to the late lamented Professor Hertz for the very able advice he at all times so readily gave me while I was engaged on the above work under him at the University of Bonn during the session 1892—93.

"On the Leicester Earthquake of August 4, 1893." By CHARLES DAVISON, M.A., F.G.S., Mathematical Master at King Edward's High School, Birmingham. Communicated by Professor J. H. POYNTING, F.R.S. Received February 28,—Read May 10, 1894.

On August 4, 1893, at 6.41 P.M. (G.M.T.), an earthquake shock was felt throughout the whole of Leicestershire and Rutland, and in parts also of the adjoining counties of Lincoln, Nottingham, Derby, Stafford, Warwick and Northampton. The disturbed area, therefore, lies entirely within the land. It is also one over which villages and country houses are for the most part closely scattered, and it has thus been possible to obtain a large number of careful and detailed accounts. I have received altogether 391 records from 298 places where the earthquake was observed, and 103 others from 97 places where, so far as known, no trace of it was perceived.*

My inquiries were carried out on the supposition that tectonic earthquakes are, as a rule, mere incidents in the gradual development of faults, that the shock is caused by the friction which results from one rock-mass slipping slightly but heavily over and against the other, the accompanying sound and tremulous motion being due to the exceedingly small and rapid vibrations which proceed chiefly from the margins of the fault-surface over which the slip takes place.†

The interpretation of the evidence collected rests on the following principles:—

(1.) The direction of the fault is parallel, or nearly so, to that of the longer axis of the disturbed area, or of an isoseismal line. (2.) The intensity of the shock increases in both directions from the fault-line until a maximum is reached, and then decreases, so that on both sides of the fault, and close to it, there is a meizoseismal area;[*] but the maximum intensity is greatest in the area situated on the side towards which the fault hades. (3.) Other conditions being the same, isoseismal lines which can be traced on both sides of the fault are further apart on the side towards which the fault hades. (4.) The nature of the fault-slip may be ascertained to a certain extent by the relative positions of the isoseismal lines, and by the time- and space-relations of the sound and shock.

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* The expenses of the inquiry were defrayed by part of a grant which I had the honour to receive from the Government Research Fund. I regret that I am unable to acknowledge in detail the valuable and courteous assistance rendered by my numerous correspondents.

† "On the Nature and Origin of Earthquake-sounds," 'Geol. Mag.,' vol. 9, 1892, pp. 206—218.

Disturbed Area and Isoseismal Lines.

On the accompanying map are shown the principal places within the disturbed area and those to which reference is made in this paper. The three curves are isoseismal lines corresponding to intensities 5, 4, and approximately 3 of the Rossi-Forel scale. The latter may be regarded as practically the boundary of the disturbed area, though records have come from four places outside it where the shock was very slightly felt. In drawing this line the knowledge of the places where no trace of the earthquake was perceived has been of considerable service.

The disturbed area is 58 miles long and 46 miles broad, and contains about 2066 square miles. The isoseismal 4 encloses an area 47 miles long, 34 miles broad, and of about 1251 square miles; that included by the isoseismal 5 is 17¼ miles long, 12 miles broad, and of about 163 square miles. The directions of the longer axes of the three curves are about W. 38° N. and E. 38° S., W. 40° N. and E. 40° S., and W. 31° N. and E. 31° S., respectively, the second probably being the most accurate. The longer axis of the isoseismal of intensity 4 lies on the north-east side of that of the isoseismal 5, and we therefore conclude that, if the earthquake be fault-formed, the direction of the fault must be about W. 40° N. and E. 40° S., and its hade must be to the north-east.

The principal meizoseismal area is a narrow band, nearly parallel to the longer axis of the isoseismal 5, and at a short distance from it on the north-east side. It extends from Sheepshed to the neighbourhood of Rothley, a distance of eight miles. The intensity was greatest at and near Woodhouse Eaves.

On the opposite side of the axis of the same isoseismal there is another, though less distinctly marked, meizoseismal area, which includes Markfield, but the intensity here was less than in the north-east meizoseismal area.

This confirms the previous conclusion that the fault must hade to the north-east. It also shows that the fault-line must pass between

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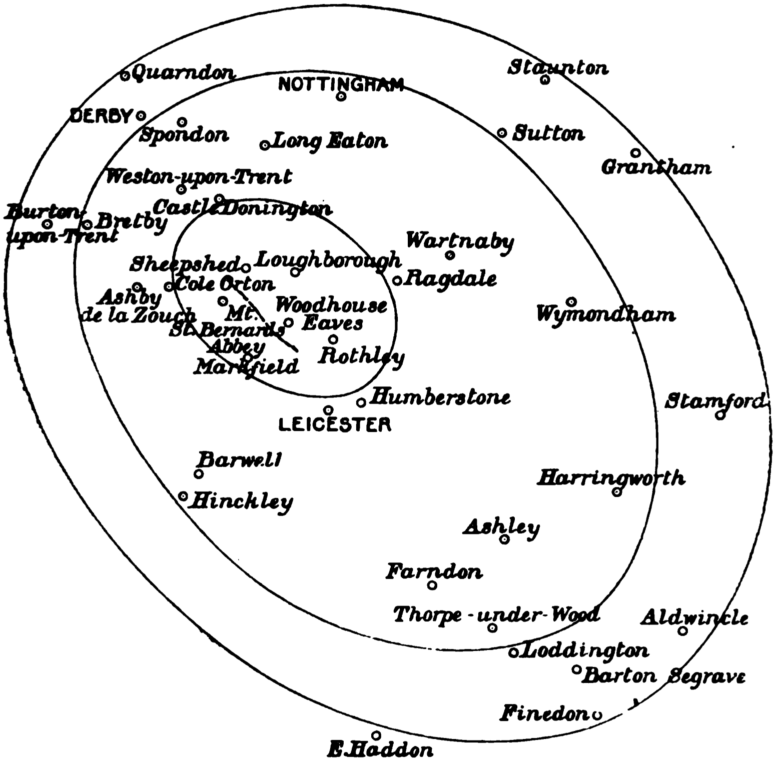
[*] The term "meizoseismal area" is here used as denoting an area where the intensity is greater than in the surrounding region.



Area of the Earthquake of August 4, 1893.

Woodhouse Eaves and Markfield. The position of the fault is thus approximately determined.

Geology of the Epicentral District.

The rocks of Charnwood Forest have been described by Messrs. Hill and Bonney in a valuable series of memoirs.* The district is now being re-examined by Mr. C. Fox Strangways, of H.M. Geological Survey, to whom I am indebted for some additional information.

The part of the Forest region with which we are immediately concerned is that lying near the centre of the isoseismal 5. Here the beds, which are believed by Messrs. Hill and Bonney to be of pre-Cambrian age, "are elevated in an elongated dome-shaped anticlinal of which only one-half is visible, and the vertex points to the south-east; so that denudation has exposed the lowest beds at the northern end; and the others lap round them in rudely elliptical zones." Along the anticlinal axis, the direction of which is approximately north-west and south-east, but in the neighbourhood of Woodhouse Eaves more nearly W. 40° N. and E. 40° S., there appears to run a considerable fault, the beds on the south-west side being older than those on the north-east. The course of this supposed fault is indicated on the map by a dotted line. If the fault be a normal one, but of this there is no certainty from geological evidence, it must clearly hade to the north-east.

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* "The Precarboniferous Rocks of Charnwood Forest," 'Quart. Journ. Geol. Soc.,' vol. 33, 1877, pp. 754—789; vol. 34, 1878, pp. 199—239; vol. 36, 1880, pp. 337—350.

The close correspondence between the probable position of this fault and that of the fault suggested by the seismic evidence allows us to infer, I think, with some degree of confidence, that the Leicester earthquake was caused by a slip of the anticlinal fault of Charnwood Forest.

Nature of the Earthquake Phenomena.

In the present section I will first give a few brief descriptions of the earthquake, and will afterwards present a summary of the evidence from different points of view. The first group of accounts are from places near the major axis of the disturbed area, the second from places near the minor axis of the isoseismal 5, which may be regarded as the minor axis of the disturbed area.

1. (*a.*) Accounts from places between the north-west end of the major axis and the isoseismal 5: Quarndon, a dull rumbling sound first heard, as of some very distant explosion, lasting about five seconds, followed without any interval by a quick tremulous motion, which gradually increased in intensity, and then died away, intensity 3. Derby, a tremulous motion, increasing in intensity for three seconds, and then decreasing until it ceased after one second more, intensity 3; a soft, slightly tremulous sound heard for two seconds before the tremulous motion began, increasing in intensity and ending rather abruptly just before the tremulous motion was strongest. Spondon, a tremulous motion for two or three seconds, preceding more prominent vibrations which lasted for two seconds more and were not followed by tremulous motion; a rumbling sound like that of a heavy cart passing close to the house, the beginning and end of which coincided with, or slightly preceded, the beginning and end of the shock respectively, the sound loudest when the vibrations were strongest. Weston-on-Trent, a slight tremulous motion for about two seconds, succeeded by two series of vibrations, each

lasting about two seconds, not followed by tremulous motion, intensity nearly 5; the beginning of the sound coincided with that of the shock, it began abruptly, and died away a second or two before the end of the shock. The most interesting point brought out by these observations is that, as the earth-wave radiated from the centre, the sound apparently outraced the shock, leading at first sight to the conclusion that the velocity of the sound was greater than that of the shock. In a later section it will be seen, however, that such a conclusion would be incorrect.

(*b.*) Accounts from places within the isoseismal 5: Mount St. Bernard's Abbey, prominent vibrations, followed by tremulous motion, duration about five seconds; the beginning of the sound slightly preceded that of the shock, the end of the sound followed that of the shock by at least three or four seconds. Sheepshed, the vibrations gradually increased in intensity and then died away, duration about seven seconds; the beginning of the sound preceded that of the shock by about two seconds, the end of the sound followed that of the shock by about two seconds, the sound loudest at the time when the vibrations were strongest. Markfield, prominent vibrations, preceded and followed by tremulous motion, duration about five seconds; the beginning of the sound slightly preceded that of the shock, the end coincided approximately with that of the shock, the principal vibrations felt at, or immediately after, the instant when the sound was loudest. Thus, at places close to the epicentrum the sound generally overlapped the shock at both ends, and there was little or no interval between their epochs of maximum intensity.

(*c.*) Accounts from places between the isoseismal 5 and the south-east end of the major axis: Humberstone, principal vibrations for three or four seconds, strongest near the beginning, followed by tremulous motion for two or three seconds, intensity 4; the beginning and end of the sound coincided approximately with those of the shock. Harringworth, tremulous motion, followed by principal vibrations, intensity 4; the beginning of the sound preceded that of the shock, the ends of both coincided, the principal vibrations felt after the instant when the sound was loudest. Barton Segrave, the sound, a deep loud rumble, lasting four or five seconds, seemed to cease as the vibrations began; these were strongest near the middle, and lasted about five seconds, intensity 3. In this direction also, then, the sound apparently outraced the shock.

2. Sutton (near the north-east end of the minor axis), one series of vibrations, strongest towards the end, duration five or six seconds, intensity 3; the beginning of the sound preceded that of the shock by two or three seconds, the end followed that of the shock, the principal vibrations felt when the sound was loudest. Barwell (near the south-west end of the minor axis), principal vibrations for about

four seconds, followed by tremulous motion for about eight seconds; the beginning of the sound coincided with that of the shock, the end followed that of the shock by about two seconds, the principal vibrations felt when the sound was loudest. Both of these places are at considerable distances from the fault-line, and on opposite sides of it. Consequently, from these and similar observations, we may conclude that in the direction of the minor axis there is no sign of the sound outracing the shock.

Time-Relations of the Sound and Shock.—The beginning of the sound preceded that of the shock at 59 places, coincided with it at 11, and followed it at 3, places. The latter are, however, close to places where the sound was heard before the shock. Hence, in all parts of the disturbed area, the beginning of the sound as a rule preceded that of the shock.

The end of the sound preceded that of the shock at 16 places, coincided with it at 6, and followed it at 16, places. In the majority of cases, the end of the sound preceded that of the shock at places near the major axis, and especially close to the ends of it; and it followed the end of the shock at places chiefly within the isoseismal 5 and in the neighbourhood of the minor axis.

The epoch of maximum intensity of the sound preceded that of the shock at 19 places, which are as a rule near the ends of the major axis, coincided with it at 18 places, which are not far distant from the epicentrum or minor axis, and followed it at 3 places, namely, Rothley, Woodhouse Eaves and Wymondham.

Lastly, the sound appeared to precede the shock entirely at seven places, Ashley, Barton Segrave, Bretby, Farndon, Hazelbeach Hill, Quarndon and Wartnaby. Six of these are close to the ends of the major axis, and still closer to the continuation of the anticlinal fault-line.

Thus, the more detailed evidence here summarised supports the conclusion previously stated, that the sound apparently outraced the shock in the direction of the major axis of the disturbed area, but not in the direction of the minor axis.

Space-Relations of the Sound and Shock.—In the absence of any scale of seismic sound-intensity, it has not been possible to indicate the position of any isacoustic lines. It is clear, however, that they would not be concentric with the isoseismal lines. For instance, the shock was felt, while no sound was heard, by the observers at Burton-on-Trent, Castle Donington, Cole Orton, Long Eaton and Staunton. The sound was heard, while no shock was felt, by the observers at Aldwincle, East Haddon, Finedon, Holme, Loddington, Ragdale Hall and Thorpe Underwood. In one or two cases this may have been due merely to defective observation; but, on the whole, it is evident that the shock was a more prominent feature than the sound near the north-west end of the major axis, while the sound was more prominent than the shock near the south-east end.

Origin of the Earthquake.

Whatever may be the extent of the fault-surface over which a slip takes place, there must, in the simplest case, be a certain central region over which the slip is comparatively great, surrounded by a marginal area over which it is very small, and gradually diminishes in all directions until it disappears along the boundary. The minute and exceedingly rapid vibrations proceeding chiefly from this marginal area are, I believe, those which are perceptible as sound; the sound which precedes, accompanies and follows the shock being produced by the vibrations which come from the nearer lateral margin, the upper margin chiefly, and the further lateral margin, with reference to the place of observation.

In the case of the Leicester earthquake, the sound was observed before the shock all over the disturbed area, because no part of that area was at a great distance from the nearer lateral margin of the seismic focus. But some places, especially near the end of the major axis, were so far from the further lateral margin that no sound following the shock could be heard.

At a place (like Quarndon) close to one end of the major axis, the sound-vibrations from the further edge of the nearer lateral margin were nearly or quite imperceptible, and the sound entirely preceded the shock. A little nearer the epicentrum (at Derby) the sound-vibrations from the nearer part of the upper margin were perceptible, and consequently the sound partly overlapped the shock, but the beginning, epoch of maximum intensity, and end of the sound preceded respectively those of the shock. Lastly, at places close to the epicentrum the sound-vibrations from the further lateral margin could be perceived, and thus the sound was heard both before, during and after the shock, while the epochs of their maximum intensity were nearly or quite coincident.

In the neighbourhood of the minor axis the sound was heard at the same time as the shock, and generally both before and after. No interval was apparent between their epochs of maximum intensity, for the places of observation were at nearly the same distance from the upper lateral margin and central area of the seismic focus.

The time-relations of the sound and shock are thus readily explained on the supposition that the sound-vibrations come chiefly from the marginal area of the seismic focus. But this theory of their origin receives a stronger support from the observations summarised above than its mere capability of offering a satisfactory explanation. The sound was heard before the shock practically at all parts of the disturbed area; it was heard after the shock at places near the ends of the minor axis but not of the major axis. This shows, I think, that the preliminary and subsequent sounds must have been due to vibrations proceeding from two distinct origins, situated roughly in a north-west and south-east line.

Length of the Fault-Slip.—On this subject our conclusions are of necessity only approximate. It may be shown, however, that the seismic focus must have been several miles in length.

The elongated form of the disturbed area and of the isoseismal lines is sufficient evidence of this. If the slip were instantaneous, the length of the focus would probably be greater than the difference between the length and breadth of the disturbed area, *i.e.*, than about 12 miles.

Again, the curve which includes all places on the north-west side of the minor axis at which the sound preceded the shock, and all those on the south-east side at which it followed the shock, bounds the area over which the sound-vibrations from the north-west lateral margin were perceptible. Similarly may be obtained the area over which the sound-vibrations from the south-east lateral margin were observed. The centres of these areas must, as a rule, lie vertically over the corresponding lateral margins, and the distance between them must therefore be less than the length of the seismic focus. It is difficult to determine the centre of the north-west area very exactly, but the length of the fault-slip, as ascertained by this method, cannot have been much less than 12 or 15 miles.

It is possible, however, that the slipping did not take place instantaneously,* and, if so, these estimates may be a little too great. But, in any case, the fault-slip must have been continued for some distance under the Triassic rocks on which Leicester is situated.

Nature of the Fault-Slip.—The intensity of the shock was greatest on the north-east side of the fault at and near Woodhouse Eaves, and on the south-west side at Markfield. The line joining these places is perpendicular to the fault-line, and we may therefore conclude that the slip was greatest at this part of the fault. For a distance of two or three miles† in either direction the slip may have been considerable, and it then died out rather rapidly in amount towards the north-west, and much more slowly towards the south-east. That the south-east lateral margin of the seismic focus was longer than the north-west is shown (1) by the relative distances between the isoseismal lines in these directions, and (2) by the greater prominence of the sound in the south-east part of the disturbed area and of the shock in the north-west part.

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* The great extension of the isoseismal 4 towards the south-east may be partly due to the fault-slip starting near Woodhouse Eaves and spreading out rapidly in either direction, but towards the south-east with a velocity not much greater than that of the earth-wave.

† The difference between the length and breadth of the isoseismal 5 is about five miles.

After two or three seconds there appears (from the double character of the shock) to have been a second and slighter slip, probably at a short distance to the north-west of the former.

History of the Charnwood Forest Fault.—The greater part of the displacement along the anticlinal fault seems to have taken place in pre-Carboniferous times, leaving only a few hundred feet to be accomplished in subsequent periods.* Earthquakes in the Charnwood district are now extremely rare. Between December 23, 1838, and August 4, 1893, we have not one recorded.† The shocks of the present century thus indicate at how slow a rate the fault is now growing. They also show that that growth has not yet ceased, and enable us to form some conception of the vast interval of time over which the life of a fault may extend.

"On the Different Forms of Breathing." By WILLIAM MARCET, M.D., F.R.S. Received June 12,—Read June 21, 1894. Revised September 7, 1894.

I must beg leave at the outset to acknowledge the valuable aid of my Assistant, Mr. R. B. Floris, F.C.S., in the present inquiry. An investigation of this kind, necessitating much consideration, can only be carried to a successful issue by arguments and discussions, and to Mr. Floris much credit is due for the light these discussions have shed on the inquiry; I feel also indebted to him for the care he has bestowed on the numerous analyses he has made for me, and for the accuracy with which his work has invariably been done.

From a general view of the function of respiration it follows that there are four fundamental forms of breathing; their characters are the following:—

1st. Normal breathing in the state of repose—a regular succession of the respiratory movements, involuntary, unattended with fatigue, and leaving an approximately unchanged balance between the CO_2 in store, in the blood, and the CO_2 expired.

2nd. Forced breathing—a voluntarily increased thoracic expansion, brought about by the contraction of muscles differing more or less from those in common use in respiration and attended with fatigue. The expiration of carbonic acid is increased beyond the normal;

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* 'Quart. Journ. Geol. Soc.,' vol. 34, 1878, p. 232.

† W. Roper, 'A List of the more remarkable Earthquakes in Great Britain and Ireland during the Christian Era' (Lancaster, 1889), pp. 34—42.

though not from excessive oxidation, with the exception of a small amount due to the extra work of the respiratory muscles.

3rd. Respiration *under exercise*—increased volumes of air inspired and expired in a given time with an expiration of CO_2 in excess of the CO_2 expired in repose, and unattended with fatigue. At the same time there occurs an increase of the CO_2 in store in the blood, which takes place with a certain regularity during the first fifteen or eighteen minutes after exercise has begun, and is irregular afterwards. This excess of stored up CO_2 is given out as soon as exercise is followed by repose.

4th. The fourth form of breathing is under the influence of volition, when exerted in a person's imagination towards a powerful muscular exertion, while the muscles are in a perfect state of repose. This form of breathing exhibits the characters of forced respiration and respiration under exercise.

These four different forms of breathing have received special attention, and will, collectively, be found to include every variety of respiration. They have been recorded graphically in the charts which accompany the present paper. By means of a drum revolving regularly by clockwork, and of a style at the end of a rod fixed to the summit of the bell-jar into which the air was expired, tracings were obtained on lithographed charts. The abscissæ on the charts indicate the litres of air expired, and the ordinates the minutes through which the experiment was continued.

The air expired was analysed, either for the determination of its CO_2 alone, or of its CO_2, O, and N; the volume of nitrogen obtained yielded by a simple proportion the volume of air inspired.

I shall now beg to give an account of my inquiry on these different forms of respiration.

1*st*. *Normal Breathing in the State of Repose.*

The methods adopted in the present work have been fully described elsewhere; as time elapsed improvements were introduced, and this last year the bell-jars used for collecting the expired air, instead of being exactly balanced as in former experiments, had their counterpoise charged in such a way as to give a slight ascending motion to the receivers when in free communication with the atmosphere, the speed they acquired being rather less than would be produced by the rush of air owing to the expiratory effort. Thus, the respiration was unattended with the unconscious strain necessary to raise the bell-jar while perfectly balanced. This effort, although unnoticed at the time, had a slight tendency to bring on after a while a sensation of fatigue in the respiratory muscles, and probably to increase, though to a very small amount, the volume of

CO_2 expired; at the same time the percentage of CO_2 in that air was somewhat greater than might have been anticipated. The present arrangement made away with that slight defect. It should be understood that the person under experiment while inspiring through the nose, first expired through a tube into the open air, then by means of three-way stop-cocks was placed in communication with either of the three bell-jars, used in these experiments; thus, the bell-jar only began ascending when the air was expired, while, during the period of inspiration, the mouth-piece retained in the mouth entirely checked any ascending tendency of the receiver.

This last season (1893–94), both I and Mr. Floris submitted to experiment for the determination of our carbonic acid expired and oxygen absorbed (not given out as CO_2) in the state of repose. In my case eleven experiments were made from 2 hours to 2½ hours after luncheon (chop, potatoes, and bread), while four others were undertaken within 1 hour and 5 minutes after lunch. The results are as follows:—

The Author, from 2 hours to 2 hours and 30 mins. after Luncheon.
Per minute.

Time after luncheon.			CO_2 expired.	O absorbed.	O consumed.
	h.	m.	c.c.	c.c.	c.c.
	2	0	216·0	32·7	248·7
	2	5	210·9	36·0	246·9
	2	0	209·4	36·3	245·9
	2	5	206·4	52·1	258·5
	2	0	200·2	45·0	245·2
	2	20	192·1	34·9	227·0
	2	30	188·7	34·0	222·7
	2	20	184·4	38·2	222·6
	2	15	175·5	27·7	203·2
	2	20	175·4	32·1	207·5
	2	0	163·5	37·6	201·1
Means..	2	10	192·9	37·0	229·9

Respiratory ratio .... $\left\{\frac{192 \cdot 9}{229 \cdot 9}\right. = 0 \cdot 839.$

The four experiments made within 1 hour and 5 minutes after luncheon are shown in the next table.

From these two series of experiments it will be observed that less CO_2 is expired during the first hour after a meal than from 2 to 2½ hours, but there is considerably more oxygen absorbed within the first hour than from 2 to 2½ hours after the ingestion of food, the proportion of 54·4 c.c. to 37 c.c. is a clear indication that such is the case.

The Author, within 1 hour and 5 mins. after Luncheon.

Time after luncheon.			CO$_2$ expired.	O absorbed.	O consumed.
	h.	m.	c.c.	c.c.	c.c.
	0	45	159·7	47·9	207·6
	0	25	203·8	55·6	259·4
	0	45	181·2	55·8	237·0
	1	5	191·8	58·3	250·1
Means..	0	45	184·1	54·4	238·5
Respiratory ratio .... $\left\{\frac{184\cdot1}{238\cdot5}\right. = 0\cdot772$					

The experiments on Mr. Floris are made partly after luncheon and partly between breakfast and luncheon. There are fifteen of the former and twelve of the latter. The experiments after lunch show more CO$_2$ expired than those made after breakfast, and considerably more O absorbed, the figure for the mean volumes of oxygen absorbed being 40·5 c.c. after luncheon, and only 22·4 c.c. after breakfast.

If we now compare with each other the present series of experiments made individually for each of us, it will be seen that in those undertaken at a mean time of 2 to 2¼ hours after luncheon, the ratio of O consumed to CO$_2$ produced is all but exactly the same, these figures being 0·839 in my case and 0·840 in that of Mr. Floris. Moreover, if the whole of the experiments on Mr. Floris be taken into account, including those after luncheon and after breakfast, the respiratory ratio will be 0·862, which is all but exactly the figure I formerly obtained for myself, 0·864 (means fasting and under food), and near to the figure for a former assistant, 0·875, these results agreeing closely with the ratio given by Messrs. Jolyet Bergonié et Sigalas, 0·864, and by Mr. C. Speck, 0·869.

While in the experiments reported in my last paper, the volume of oxygen consumed in a certain time was nearly the same for each individual person (one excepted) under similar physical circumstances, in the present instance there is a variation between the volumes of oxygen consumed, the extreme difference amounts in my case to 47·6 c.c., in that of Mr. Floris to 30 c.c. per minute. Moreover, we no longer observe, as formerly, that the oscillations in the figures for carbonic acid expired, and corresponding oxygen absorbe vary in a measure inversely to each other. After giving the questio a full consideration, I have come to the conclusion that this is du to the method of collecting the air expired. In the former exper ments this air was expired into a bell-jar perfectly balanced unde atmospheric pressure, while in the present instance the air wa

Mr. Floris under Experiment.

After Breakfast—Fish or bacon or eggs, tea or coffee.

After Luncheon—Mutton chop, potatoes, bread.

Per minute.

Time after breakfast.		CO$_2$ expired.	O absorbed.	O consumed.
hr.	m.	c.c.	c.c.	c.c.
1	15	218·9	29·1	248·0
2	0	218·5	31·6	250·1
2	5	203·5	28·5	232·0
1	30	193·2	16·9	210·1
3	0	192·7	32·4	225·1
3	0	191·3	22·6	213·9
2	30	189·5	10·1	199·6
2	20	188·0	28·8	216·8
2	15	187·8	12·7	200·5
3	0	185·6	23·6	209·2
4	0	173·8	14·1	187·9
2	30	171·2	18·7	189·9
Means 2	27	192·8	22·4	215·3

Respiratory ratio after breakfast $\frac{192·8}{215·3} = 0·895.$

Time after luncheon.		CO$_2$ expired.	O absorbed.	O consumed.
h.	m.	c.c.	c.c.	c.c.
2	5	228·1	41·5	269·6
2	17	227·1	33·7	260·8
1	55	226·3	34·6	260·9
2	35	219·7	32·7	252·4
2	20	216·5	46·1	262·6
2	8	215·8	41·5	257·3
2	30	215·4	39·0	254·4
2	15	211·1	36·9	248·0
2	20	209·6	35·7	245·3
2	30	208·2	29·1	237·3
0	15	207·2	42·7	249·9
3	0	205·9	37·0	242·9
2	24	200·3	57·7	258·0
1	50	199·3	50·1	249·4
3	0	190·9	48·8	239·7
Means 2	15	212·1	40·5	252·6

Respiratory ratio after luncheon $\frac{212·1}{252·6} = 0·840.$

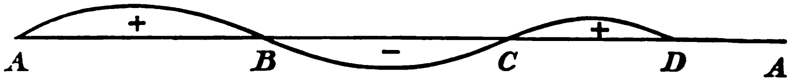
Total mean respiratory ratio $\frac{203·5}{236·0} = 0·862.$

expired into bell-jars with weighted counterpoise, as previously explained. With the balanced bell-jar there was a slight amount of work done which gave the breathing a degree of steadiness wanting with the counterpoise weighted; in the former experiments the very slight, though imperceptible, effort to raise the bell-jar gave regularity to the respiratory function. It must be recollected that during the day time, sitting perfectly quiet in an arm-chair, from a quarter of an hour to an hour, is altogether unnatural in ordinary life. We are always in a state of activity, and therefore the slight effort to raise the balanced bell-jar is perhaps more consistent with the ordinary functions of life than breathing perfectly freely, doing practically no work, indeed breathing much as we do at night in bed and during sleep. I therefore believe that the irregularity in the present figures obtained for the composition of expired air does not militate in any way against my former conclusion.

The next subject for our consideration will be

Forced Breathing.

Let us follow the different stages of this form of respiration. The person under experiment is resting in the recumbent posture and perfectly still, he then takes a succession of *deep* nasal inspirations while expiring through the mouth into one of the bell-jars. After a certain time, say one, two, or three minutes, he suddenly relapses into natural automatic breathing, and then observes that his respiration subsides for a few seconds into a pause or state of apnœa; by degrees it returns, shallow and feeble, and then fuller, increasing beyond the normal. If the apnœa has been great, the increase is well marked, but if comparatively slight the increase may be only just perceptible. These characters are invariably met with in every instance of "forced breathing" and are clearly shown by the tracings recorded in the charts. The phenomenon may be conveniently illustrated by means of the following diagram—



The horizontal line AA represents normal respiration, the curve AB the line of forced breathing, the curve BC the collapse in the respiration or apnœa, and gradual return to normal,* which it exceeds in CD, reaching finally the normal in D.

With reference to the respiratory changes in these three successive stages:—In the forced period, or from A to B, much more CO_2 is found to be given out of the blood than can be accounted for by the oxygen consumed, or, in other words, the volume of air expired exceeds in a marked degree the volume of air inspired. Where can the excess of CO_2 come from? It is evidently due to an emission from the blood of a portion of CO_2 stored up within it. This view is advocated by C. Speck in his paper, which has been referred to in my last communication. It will be shown presently that a further and more direct proof of this interesting fact can be experimentally given.

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* This return to normal only concerns the CO_2, the O absorbed takes a longer time to recover.

Until tracings of the phenomenon had been obtained on the recording drum, I thought the return to normal respiration was effected at the point C in the diagram, assuming that from B to C the blood recovered its CO_2 entirely and replenished its store; but as soon as tracings were obtained it became obvious that the final return to normal breathing failed to take place at C, and that the influence of the forced breathing continued to show itself as far as the point D.

This influence may appear consistent with the observation of C. Speck, who allies the "after stage" of forced breathing to the "after stage" of breathing under exercise; but the phenomenon appears to me to be strictly a character of forced respiration, inasmuch as it begins with apnœa, passing gradually into slightly increased breathing.

In the following table the CO_2 entered as *expired in forced breathing* is the total CO_2 expired from A to D in the diagram, and includes, therefore, the CO_2 expired in forced breathing plus the CO_2 emitted in the stage of apnœa and that obtained in the final reaction from C to D; the whole volume being calculated per minute.

It will be observed that the volumes of CO_2 in the experiments in forced breathing (including after stage) invariably exceed the volumes of CO_2 obtained in the same lapse of time during rest; this excess, which is entered in the table for each experiment, must represent the work done per minute in forced breathing.

The mean amount of this work done per minute would be equal to a combustion yielding 62·5 c.c. CO_2 for the author, and 47·5 c.c. CO_2 for Mr. Floris, corresponding to a consumption of carbon of 0·034 gram per minute for the author and 0·025 gram per minute for Mr. Floris.

The subject is further developed in the appendix to this paper.

A great deal more CO_2 is expired in *forced respiration* independently of the reaction and after stage than in ordinary breathing, and I shall now beg to demonstrate experimentally that this excess of expired CO_2 is from the store of absorbed CO_2 in the blood (with the exception of the small proportion for work done) and not a product of oxidation.

In order to attain that object, air forcibly inhaled was expired into three different bell-jars in succession, the air being diverted

Results of Experiments on Natural Breathing in Repose, and Forced or Laboured Respiration.

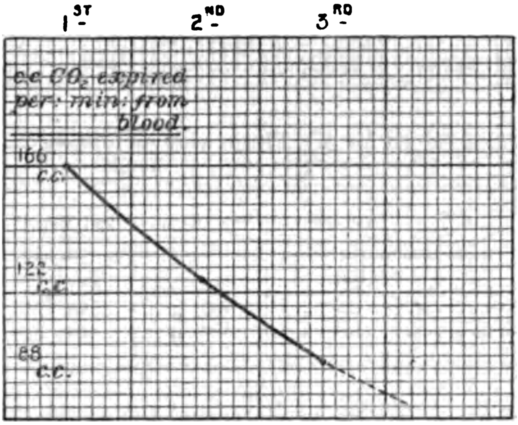
The Author under Experiment.

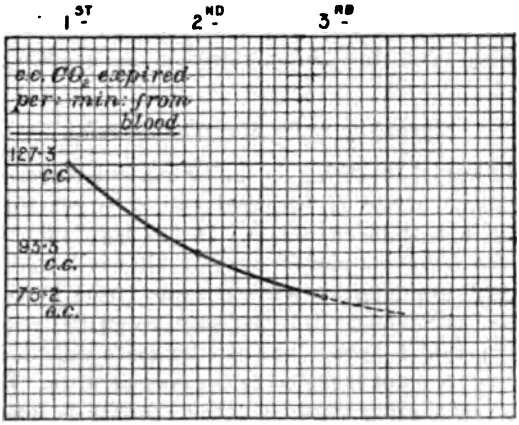
Normal respiration in repose per minute.				Forced or laboured respiration per minute.			Excess CO$_2$ expired in forced breathing.
Time after lunch.	CO$_2$ expired normal.	Vol. air expired normal.	Vol. CO$_2$ for 100 air expired normal.	CO$_2$ expired in forced breathing.	Vol. air expired in forced breathing.	Vol. CO$_2$ for 100 air expired forced.	
h. m.	c.c.	litres.	c.c.	c.c.	litres	c.c.	c.c.
2 30	188·2	4·859	3·87	253·8	6·652	3·81	65·6
0 25	203·8	5·120	3·97	311·8	8·267	3 77	108·0
2 20	192·1	4·524	4·24	262·1	6·512	4·02	70·0
2 35	191·1	4·962	3·85	245·6	5·996	4·10	46·5
0 45	159·7	4·033	3·95	204·0	5·275	3·87	44·3
2 0	200·2	4·801	4·17	240·9	5·860	4·11	40·7
Means	189·2	4·716	4·01	253·8	6·427	3·95	62·5

Mr. Floris under Experiment.

h. m.	c.c.	litres	c.c.	c.c.	litres	c.c.	c.c.
2 20 (after lunch)	209·6	4·357	4·81	251·1	5·345	4·70	41·5
2 30	215·4	4·133	5·21	270·0	5·517	4·89	54·6
2 30 (after breakfast)	187·8	3·842	4·89	229·6	4·917	4·67	41·8
1 30	193·2	3·894	4·96	248·0	5·089	4·88	54·8
2 30	189·5	3·889	4·87	234·4	5·085	4·61	44·9
Means.........	199·1	4·023	4·95	246·6	5·191	4·75	47·5

from one air holder to another by means of stopcocks; thus forced air expired in three successive periods of three minutes each was collected without undergoing the slightest loss. If the excess of CO_2 expired in forced breathing is really taken from that stored up in the blood, then it will be obvious that the second quantity of forced air expired will contain less CO_2 than the first, and the third quantity less than the second; moreover, the difference will be less and less as the experiment progresses, resulting in a curve of a parabolic form.* The experiment fully proved the correctness of the theory, three experiments for the author giving the following curves:—





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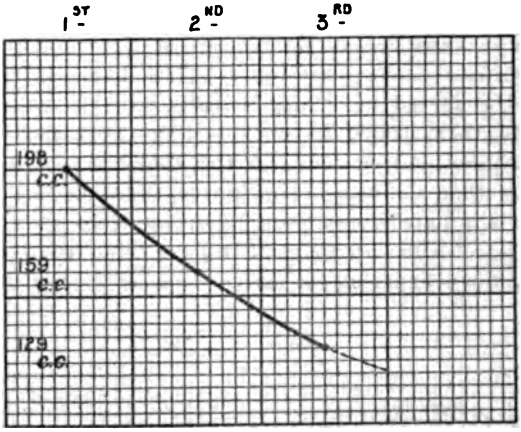
* As the volumes of CO_2 expired from the blood-store depended on the volumes of air inspired (or expired), the CO_2 expired was calculated for the mean volumes of air expired in the three different bell-jars; by this means the volumes of CO_2 obtained in those different bell-jars could be correctly compared with each other.



The results obtained for Mr. Floris were as follows:—

CO_2 emitted per minute.	1st Expt.	2nd Expt.	3rd Expt.	4th Expt.
In first three minutes..	271 c.c.	263 c.c.	327 c.c.	345 c.c.
In second ditto........	209 „	203 „	249 „	286 „
In third ditto	165 „	153 „	179 „	254 „

There can be no doubt, therefore, that the excess CO_2 expired in forced breathing is a result of diffusion through the moist pulmonary membrane of some of the CO_2 stored up in the blood. Forced breathing is concerned in sighing, yawning, and sneezing.

Breathing under Muscular Exercise.

The third form of breathing is under muscular exercise when an increased volume of air is breathed and a larger volume of CO_2 expired than in a state of repose. It has been shown in my last paper that the mean volume of oxygen, absorbed (not transformed into CO_2), at all events within a few minutes (from 15 to 18 minutes) after exercise has commenced, is the same as in the state of repose, or nearly so; this I have confirmed by more recent experiments, which, however, are not included in the present work. The increased action of the respiratory muscles under exercise is involuntary, unattended with fatigue, and differs in these respects from forced breathing. Again, in muscular exercise there is no apnœa or reaction after the exercise is arrested; on assuming a state of repose the breathing continues for a short time much the same as during exercise, and then gradually returns to the normal. This "after stage" of muscular exercise is attended with a feeling of breathlessness, due mainly to the CO_2 accumulated in the blood during exercise. By increased expansion of the lungs, it soon finds its way into the air inspired, and is thus removed from the blood.

The tracing obtained on the chart for muscular exercise is steeper than that for breathing in the state of repose; on the person under experiment assuming the recumbent posture in an armchair, the tracing will be observed to be somewhat prolonged, then it straightens, parallel with the tracing for breathing in repose.

In none of these curves are there any signs of the reaction of forced breathing.

Muscular exercise of every kind, including singing, reading, talking, laughing, weeping, gives the same kind of tracing on the chart, though of course more or less steep, as will be seen in the accompanying curves. But in such kinds of exercise as singing, reading, talking, &c., which are not followed by breathlessness, it will be seen that the curve is either not prolonged at all, or but very slightly after the exercise is over, returning quickly to the normal; and showing that in these cases the CO_2 is emitted as fast as it is formed, or nearly so, little or none being detained in the blood.

There are passive forms of exercise which also increase the volume of air breathed and produce an excess of carbonic acid, such as friction of the skin, and cutaneous irritation. On the author's bare legs being rubbed hard by two persons, the volume of air he expired increased per minute from 3·892 litres to 4·441 litres, and the volume of CO_2 from 182·6 c.c. to 208·6 c.c. It may be therefore concluded that "shampooing" is really a form of exercise, as it must be productive of increased combustion. The application of a mustard leaf to each leg, causing a powerful degree of smarting, gave rise to a similar effect on the respiration; the volume of air breathed was increased per minute in one experiment from 4·719 litres to 5·099 litres, and the CO_2 from 189·4 c.c. to 226·7 c.c.

Breathing under the Influence of the Will.

The fourth form of breathing is while under the influence of the *will.* It had occurred to me to inquire whether the *will* without muscular exercise was attended with an increased volume of air breathed and a simultaneous excess of CO_2 expired. The experiment was carried out as follows. The person under experiment rested for a while in the recumbent posture in the deck chair, and then expired naturally into the bell-jar connected with the recording instrument, thus a tracing of his normal breathing was obtained. Then he commenced applying the will as he thought best, either to carry a heavy weight, or to run up hill pursuing another person, or to ride a bicycle up hill, or in any other form, care being taken to keep the muscles absolutely at rest, when the result was invariably an increase in the volume of air expired, as shown in the chart. On suddenly dropping the influence of the will, and relapsing into the usual state of mind, a slight reaction was visible on the chart recalling forced breathing. The line, however, is straighter, and nearly free from the after rise observed in forced breathing, thus exhibiting a slight tendency to the form of curve peculiar to breathing under exercise.

Let us now inquire more closely into the present form of breathing. The moment the will is applied, as stated above, there is an increase in the volume of air breathed. What is the reason of this phenomenon? It must result from one of two different causes or from both—either an increased demand of oxygen by the blood, or the acquired habit of breathing more air while under an exercise which is itself controlled by the *will*. The increased action of the respiratory muscles when influenced by an effort of the will is *absolutely automatic*; there is no strain, no effort of any kind recalling forced breathing. Together with a greater volume of air breathed per minute we observe a marked increase of CO_2 produced and expired —an increase which amounts to a mean of 20·9 c.c. per minute. These 20·9 c.c. of CO_2 can only be due either to increased labour in the act of breathing, or increased combustion from the effect of the will, or to both these causes. The increased respiratory labour which is unconscious and unattended with fatigue must be very slight, and to my mind cannot account for 20·9 c.c. of CO_2 per minute; moreover, if the fact be taken into account that the increased volume of air breathed under an effort of the will points distinctly to a demand of oxygen from the air by the blood, and an increased production of CO_2 which has to be emitted, we shall be more and more convinced that volition (powerfully exerted) is productive of carbonic acid.

If we now turn our attention to the graphic illustration of breathing under a strong exertion of the will, we observe that the apnœa is somewhat less marked than in forced breathing, the curve straightening and thus approaching that of respiration under muscular exercise.

Taking into account the whole of the above considerations, it appears to me that we are justified in concluding, as it were from circumstantial evidence, in the absence of direct proof, that oxygen is absorbed and carbonic acid formed in the body from the mental effort of the exertion of the will. A remark falls from Dr. Vaughan Harley, in a recent paper on "The Value of Sugar and the effect of Smoking on Muscular Work"* in keeping with the foregoing observation. He noticed that if he thought about the exercise to which he was going to apply his fingers, the maximum power was obtained at the very outset. The reason of this observation is obvious. The exercise of the will caused him to take into his lungs an increased volume of air, and on that account he was the better prepared for the exercise the moment it was commenced.

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* 'Journal of Physiology,' 1894.

Professor Ramsay, of University College, kindly consented to submit to the experiment, and others were made on myself and Mr. Floris. These experiments all give the same result, that breathing under the influence of the will is a phenomenon attended with an increased volume of air breathed, and a greater volume of carbonic acid expired.

The results of these experiments are given in the following table:—

Breathing under the Influence of the Will.

Per minute.

Persons under experiment.	Vol. air expired normal.	Vol. air expired under will.	CO_2 expired normal.	CO_2 expired under will.	Time the will was applied.
	litres	litres	grams c.c.	grams	m. sec.
Prof. Ramsay.	3·698	4·078	148·0	168·9	2 35
Author.......	3·910	4·345	182·3	208·5	
„	4·173	4·478	171·4	188·1	2 25
„	3·948	4·160	156·8	169·3	2 18
Mr. Floris....	3·386	4·500	166·8	205·4	
Author.......	4·359	4·958	186·0	205·6	2 0
Mr. Floris....	3·472	3·747	185·4	200·0	1 0
Author.......	4·347	5·362	190·9	214·0	4 0
Means......	3·912	4·453	173·4	194·3	—

Mean increase in vol. air expired per min. .... 0·541 litre.
Mean increase in CO_2 expired „ .... 20·9 c.c.

The well known interesting experiment of Professor Mosso, of Turin, might be here recorded, which demonstrates in the most conclusive manner that the brain, from fatigue, loses its power to excite muscular contraction; this experiment also favouring the conclusion that brain work is attended with combustion.

The following is a summary of the present inquiry:—

There are four distinct forms of breathing, which include every variety of respiration.

1. Natural breathing in the state of repose.
2. Forced breathing:—a voluntary increased action of the thoracic

muscles attended with fatigue and with an expiration of carbonic acid increased beyond the normal. This, however, is due mainly to the displacement of carbonic acid present normally in the blood, a small proportion of CO_2 only being the result of the work done by the action of the muscles concerned in the forced respiration. A sudden return from forced to normal breathing in repose is attended with an involuntary double reaction, the first being a respiratory pause, and the second a slight increase of the respiration more or less marked according to the intensity of the first reaction and merging into natural breathing.

3. Respiration *under exercise*, exhibiting characters shown in the tracings, and in other respects described in a previous paper.

4. Breathing *in a state of muscular repose while under the influence of a strong effort of volition.* The characters of this form of respiration partake of breathing *under muscular exercise* and of *forced breathing.* Like breathing during exercise, the volume of air respired is increased, and the action of the respiratory muscles is automatic and unattended with any fatigue, while there is an elimination of an increase of CO_2. Like forced breathing, on releasing the influence of the will there is a slight reaction or pause, but it is distinctly less marked than after forced breathing, and the following after stage observed in forced breathing, is barely, if at all perceptible. From these circumstances it is concluded, though as it were from circumstantial evidence, that volition strongly exerted is productive of the formation of carbonic acid.

The second form of breathing, or forced expiration, includes sneezing, sighing, and yawning.

The third form of breathing, or under exercise, includes what may be called vocal breathing, or reading, talking, singing, weeping, laughing, shouting, and coughing. In these instances there is but little or no CO_2 absorbed in the blood as it is given out as the exercise proceeds.

The fourth form of breathing may be considered as including every kind of mental exertion.

Finally, the different forms of breathing may assist or clash with each other. Thus forced breathing assists breathing under exercise, especially when repose follows exercise.

Forced respiration may clash with vocal respiration as shown by the difficulty experienced in talking or singing immediately after forced breathing.

Appendix.

An explanation is wanted with reference to the production of heat required for the labour of forced breathing. It might be objected in accordance with the statement of Hirn, in his book on the " Mechanical Theory of Heat," that the heat emitted by an animal should be measured by the amount of oxygen consumed, while I look upon this heat as due exclusively to the CO_2 produced.

The fact contained in my last paper that the mean volume of oxygen absorbed (not transformed into CO_2) in exercise, is the same as that absorbed in the state of rest, shows clearly that the oxygen absorbed is not concerned in the production of heat necessary for muscular exercise.

By an inspection of the following table, it will be clearly seen that the oxygen "absorbed" (not transformed into CO_2) under forced breathing was very much less than that absorbed in natural respiration, instead of being in excess, as might have been expected if the oxygen "absorbed" was concerned in the production of the heat required for the labour of forced breathing. Subsequent investigation showed that the reduction of the oxygen "absorbed" in forced breathing was due to the fact that, although the CO_2 had returned to the normal in the above experiments, still the O "absorbed" was short

Oxygen absorbed and Carbonic Acid Expired at Rest, Normal and Forced.

The Author under Experiment.

	Normal in repose.			Under forced breathing.		
	CO_2 expired.	O absorbed.	O consumed.	CO_2 expired.	O absorbed.	O consumed.
	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.
	203·8	55·6	259·4	311·8	nil	352·0
	192·1	34·9	227·0	262·1	11·2	273·3
	191·1	34·6	225·7	245·6	14·0	259·6
	159·7	47·9	207·6	204·0	45·6	249·6
	200·2	45·0	245·2	240·9	6·4	247·3
Means ..	189·2	43·6	233·0	253·0	15·4	272·0
Mr. Floris under Experiment.						
	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.
	209·6	35·7	245·3	251·1	3·1	254·2
	215·4	39·0	254·4	270·0	35·8	305·8
	187·8	12·7	200·5	229·6	nil	225·5
	193·2	16·9	210·1	248·0	16·6	264·6
	189·5	10·1	199·6	234·4	5·6	240·0
Means ..	199·1	22·9	222·0	246·6	12·2	258·0

of its normal figure; by prolonging the experiment this figure was recovered, or very nearly so.

The following is a list of the curves on the charts which accompany this paper:—

Forced Breathing.

The Author.	Normal, forced, and reaction.

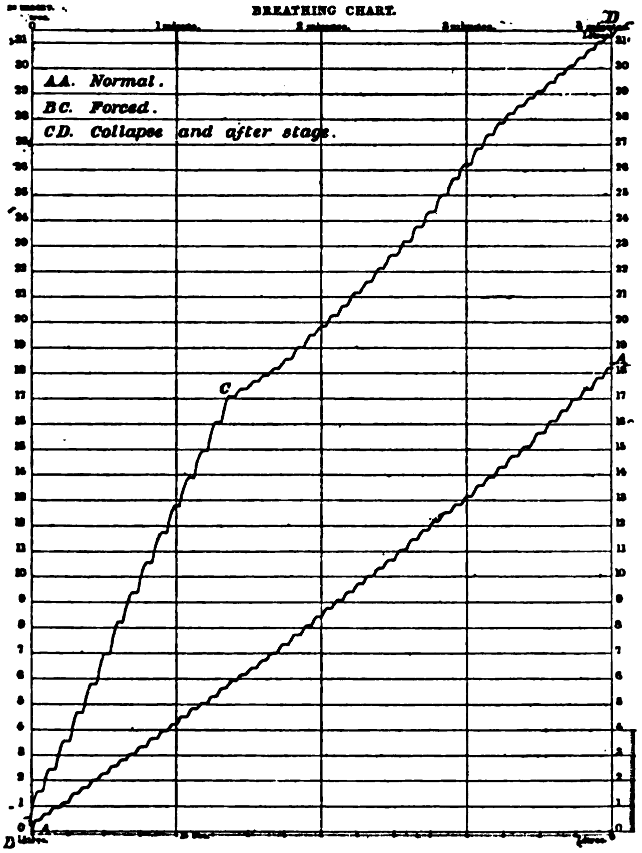
Breathing under Exercise.

The Author,	1.	Normal, exercise, and after stage, and forced to compare with exercise.
Mr. Floris,	2.	Normal, reading aloud, and after stage.
,,	3.	Normal, laughing, and after stage.

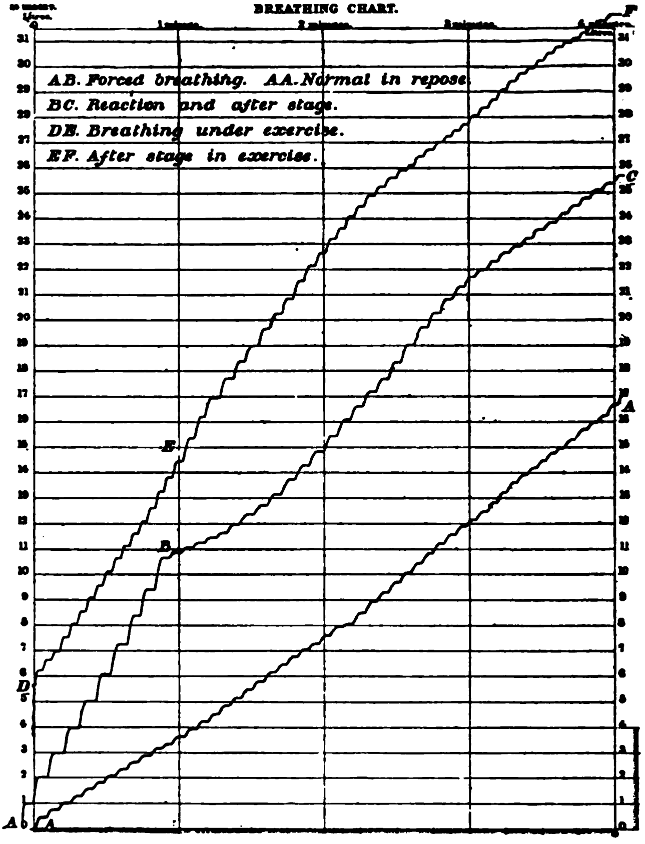
Breathing under the influence of the Will.

Mr. Floris,	1.	Normal, will, and after stage. Forced breathing to imitate will and after stage.
The Author,	2.	Normal, will, and after stage, exercise and after stage.

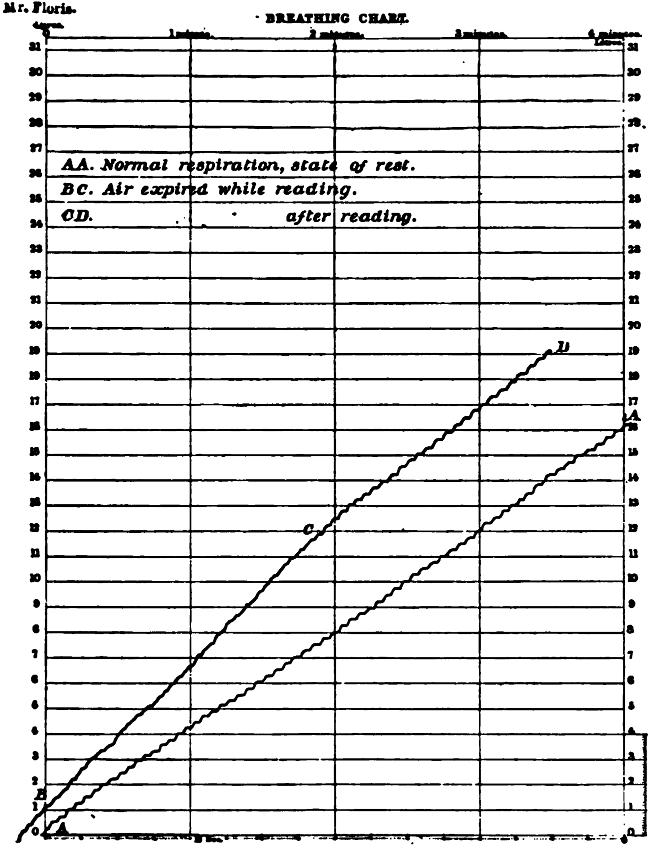
Forced Breathing.



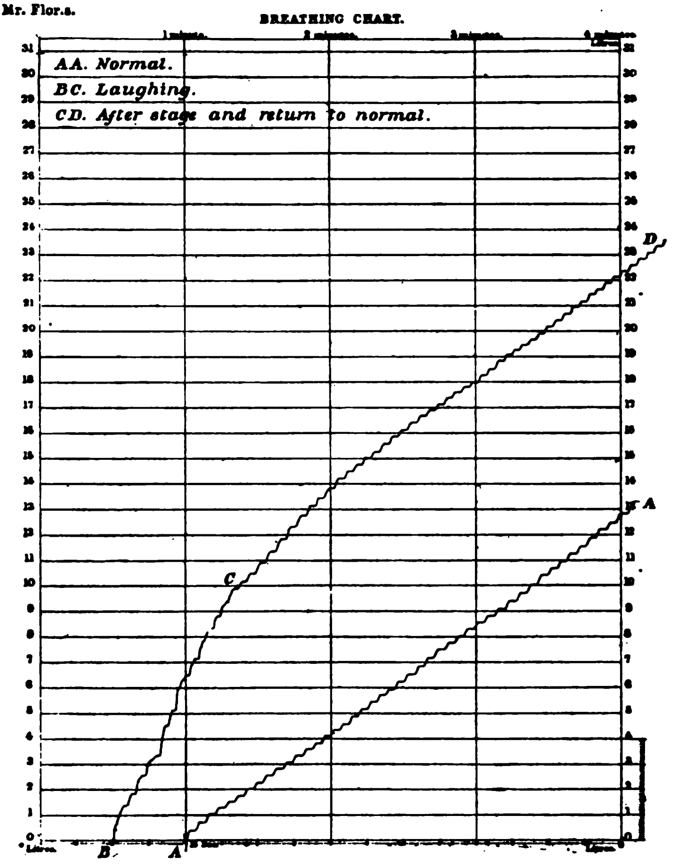
Breathing under Exercise (stepping), compared with Forced Breathing.



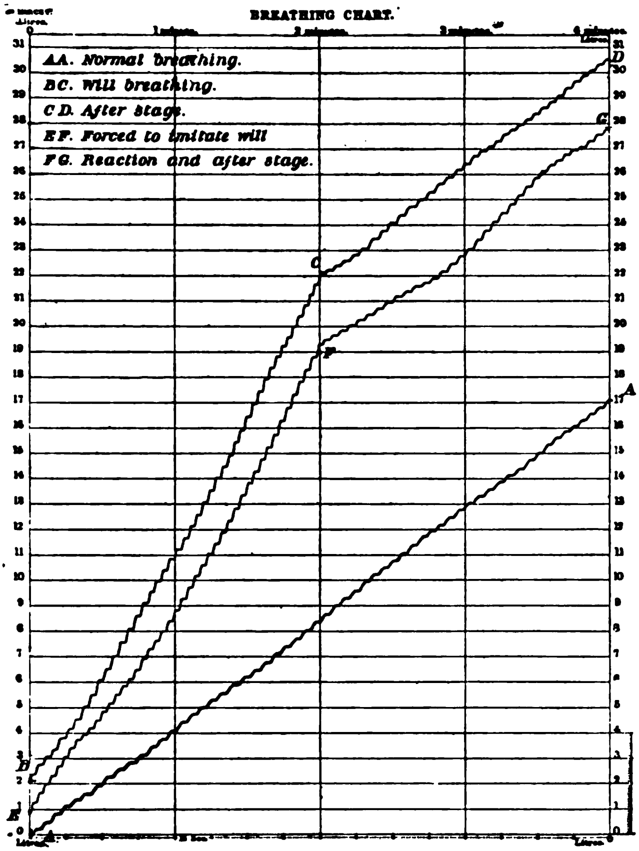
2.—Breathing under Exercise (reading aloud)



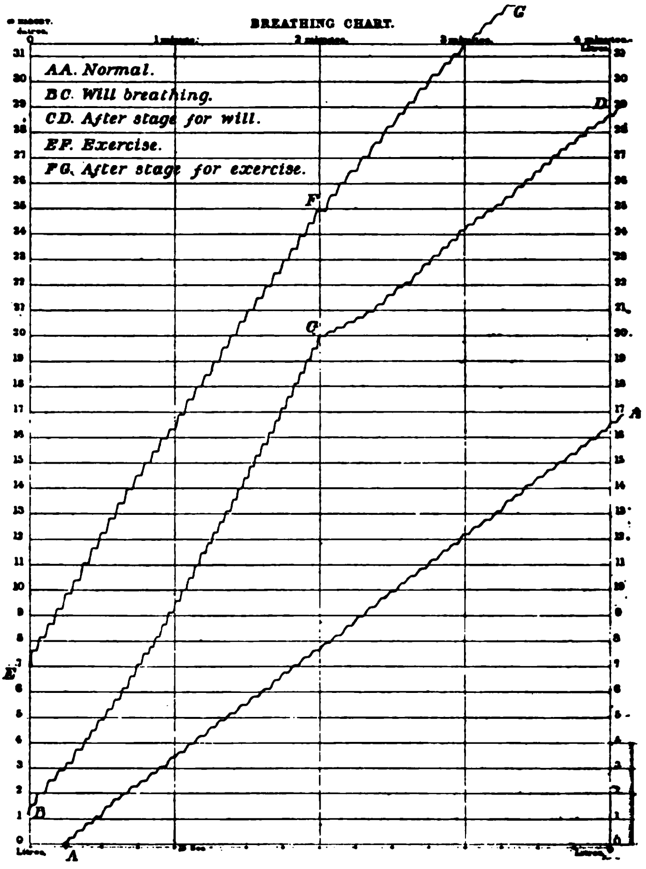
3.—Breathing under Exercise (laughing).



1.—Breathing under the Influence of the "Will," with Forced Breathing to imitate "Will."



2.—Breathing under the Influence of the Will, with Breathing under Exercise.



"On the Absorption Spectra of Dilute Solutions." By THOS. EWAN, B.Sc., Ph.D., 1851 Exhibition Scholar in Chemistry in the Owens College. Communicated by Professor H. B. DIXON, F.R.S. Received April 7,—Read June 21, 1894.

The changes which occur in the colour of a solution when its concentration is changed, are closely connected with the simultaneous changes which occur in the molecular condition of the dissolved substance, and a study of these changes of colour may be expected therefore to furnish valuable information as to the molecular condition of the dissolved body. In a paper published in the 'Philosophical Magazine' for 1892, I described some experiments on the absorption spectra of solutions of copper salts, which were made from this point of view. It was found that with increasing dilution the spectra of the different salts examined tended to become the same. The solutions experimented with were, however, tolerably concentrated, which makes the interpretation of the results difficult, as, according to Kundt's rule, the absorption spectrum of a substance varies to some extent with the dispersive power of the solvent in which it is dissolved, and, therefore, when working with strong solutions there is always the possibility that a change in the absorption, produced by changing the concentration of the solution, may be due to the alteration in dispersive power. In order to avoid this uncertainty, I have confined my attention almost exclusively to dilute aqueous solutions. In such solutions—differing very little from pure water—a change in colour can only be ascribed, according to our present knowledge, to some change in the nature of the dissolved substance, and therefore definite information as to the latter may be obtained.

A very large number of observations on the spectra of dilute solutions of salts of the same metal with different colourless acids and of the same acid with different colourless metals have been made by Ostwald ('Zeit. Phys. Chem.,' 9, 579, 1892). He measured the position of the absorption bands in the spectra, and also photographed them. Both methods of observation showed that the absorption spectra of all salts containing the same coloured metal or acid were identical. This is a necessary consequence of the electrolytic dissociation theory of Arrhenius. For at the concentrations employed, the salts being almost completely dissociated into their ions, the colour of all the salts containing the same coloured metal, for example, will be due to the same ion.

As, however, no photometric measurements of the absorption spectra of very dilute solutions have been made from this point of view, I thought it not superfluous to make a series of measurements with solutions of copper salts. It was found that the solutions of copper bromide, chloride, nitrate, and sulphate possess—within the limits of experimental error—the same absorption spectrum, while that of copper acetate is different. The difference, however, diminishes with decreasing concentration.

The absorption spectrum of the dilute solutions of copper salts differs but little from those of concentrated solutions of copper sulphate and nitrate, although in the latter the salts are only partly dissociated into their ions, and in the former much more completely. Observations of a similar nature have led Magnanini* to express the opinion that the absorption spectrum of a substance in solution is to be regarded as independent of the extent to which it is electrolytically dissociated. A more probable explanation is, as Ostwald has pointed out, that the absorption spectrum of the undissociated molecule is very nearly the same as that of the ions, and therefore a change in the degree of dissociation will produce little alteration in the spectrum.

A case in which the absorption of the undissociated molecule differs considerably from that of its ions was also investigated by Magnanini.† Solutions of the salts of violuric acid, in which the salt is almost completely dissociated, possess a violet colour; while Magnanini found the solutions of the free acid to be nearly colourless, although some 8 per cent. of the acid was dissociated into its ions. J. Wagner,‡ however, did not succeed in obtaining colourless solutions of the acid. He found that they had the same colour as the solutions of the salts, and a comparison of the intensity of colour in a solution of the free acid and in one of the sodium salt, showed that the intensity of the colour was nearly proportional to the number of ions present.

In dinitrophenol I have found another case of this sort. The solution containing only undissociated molecules is almost colourless, while solutions containing dissociated molecules are bright yellow. From the measurements of the absorption spectra of these solutions I have been able to calculate the fraction of the dinitrophenol dissociated into its ions at different concentrations, and the numbers so obtained are in very satisfactory agreement with those calculated from the electrical conductivity.

Besides the changes of colour which may be ascribed to the electrolytic dissociation of the dissolved substance, changes of colour occur in consequence of its hydrolysis by the water. As an example of this kind of change, I have studied the ferric salts, especially ferric chloride. The results are, however, more complicated than those obtained with bodies which are only electrolytically dissociated, and it will be better to consider them along with the experimental numbers. Changes of colour may also be due to the breaking up of polymeric molecules into smaller ones. It was found that solutions of colloid ferric hydroxide, obtained by dialysis, absorb less light than those obtained by the hydrolytic decomposition of ferric chloride, and in this case there is some reason to suppose that the dialysed hydroxide consists of larger molecular groups than that formed in the very dilute solution of ferric chloride.

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* Magnanini, 'Rendic. Acc. Lincei,' (5), vol. 2, p. 17, 1893; Magnanini and Bentivoglio, 'Rendic. Acc. Lincei,' vol. 7, p. 356, 1891.

† 'Zeit. Phys. Chem.,' vol. 12, p. 56, 1893.

‡ Wagner, 'Zeit. Phys. Chem.,' vol. 12, p. 314, 1893.

Apparatus Employed.

The measurement of the quantity of light of given wave-length which is absorbed by a substance in very dilute solution, is not very easy. In order to compensate for the small absorbing power of the solutions, it is necessary to employ long layers of them, and this makes it almost impossible to use a spectrophotometer such as that of Vierordt. The apparatus shown in fig. 1 was therefore put together.* Both for advice and assistance in its construction, and for the loan of valuable apparatus, I am much indebted to Professor A. Schuster, F.R.S. My thanks are also due to the Government Grant Committee of the Royal Society, who provided me with the means of obtaining several pieces of apparatus.

Fig. 1 is a diagramatic representation of the arrangement of the parts of the apparatus as seen from above.

It consists of a collimator A, with two lenses, *a* and *b*, of 20 in. focal length, which are both filled with light from the slit *c*. The source of light was an incandescent "Auer" lamp; and in order to distribute the light more evenly a plate of ground glass was introduced between the lamp and the slit. When it was desirable (as in the violet and extreme red ends of the spectrum) to obtain a stronger illumination this was removed. The parallel beam of light from the lens *a* passes through the solution; that from *b* was used as the standard of comparison; the object of using only one source of light being to keep the ratio between the brightness of these two beams constant. The solutions were contained in tubes which rested on Y-shaped supports capable of sliding along the bar C, D, which was clamped to the board on which the apparatus rested, and which allowed of a horizontal adjustment of the position of the tube. The Y-supports allowed of a vertical adjustment. The ends of the tubes were ground as nearly as possible parallel to each other, and closed by plates of glass which were simply pressed against the ground ends, no cement or washer being necessary. The tubes were 34—5 mm.

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* A description of a very similar instrument by Lummer and Brodhun appeared in the 'Zeitschrift für Instrumentenkunde,' 1892, p. 133, when my apparatus was nearly completed.

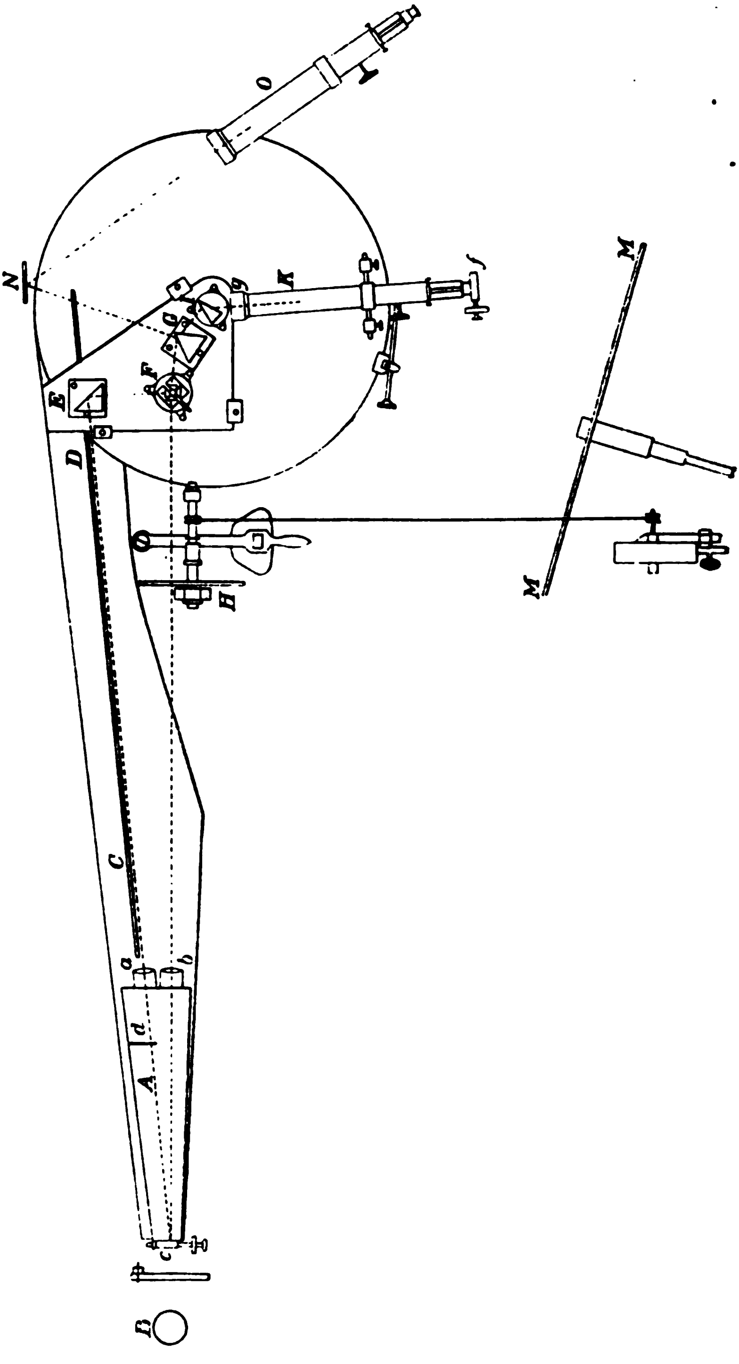
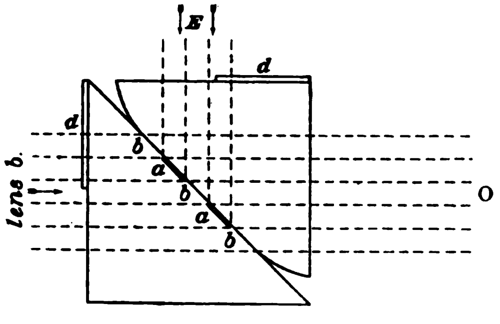


Fig. 1.

internal diameter, and the longest (length = 1 metre) was provided with five rings of ebonite (2—3 mm. wide), distributed at equal distances along its inside in order to prevent reflexion from the walls. The shorter tubes were provided with black paper rings at their ends for this purpose. After passing through the solution, the light is reflected internally by a right-angled flint-glass prism E, and falls on the Lummer and Brodhun prism F.[*] The construction of this photometric prism has been described minutely by Lummer and Brodhun ('Zeitschrift für Instrumentenkunde,' 1889, p. 461; and 1892, p. 46). It consists of two right-angled prisms, ground and polished together on their hypothenuse surfaces, and held together as shown in fig. 2. Certain parts of the surface of one prism are removed, at *a*, *a* for example, so that the light coming from E is totally reflected at the surfaces *a*, but transmitted at *b*. The light from lens *b* is only transmitted at the surfaces *b*. Consequently, an eye placed at O sees the surface *b*, *b*, *b*, *a*, *a*, illuminated on the parts marked *a* by light from E, and on the parts *b* by light from the lens *b*. The measurement then consists in varying the brightness of the light coming from lens *b*, until the brightness of the different surfaces is the same. By means of glass plates at *d*, *d* (fig. 2), a reading by equal contrast between two pairs of surfaces of somewhat different brightness, can also be made at the same time. For the details of this arrangement, however, I must refer to Lummer and Brodhun's original papers.

FIG. 2.



The variation of the intensity of the light from lens *b* was produced by means of the rotating sector described by Abney ('Phil. Mag.,' vol. 27, p. 62, 1889). The sector was made by Mr. A. Hilger.

After passing through the prism F, the two beams of light follow the same path through the two colourless flint-glass prisms G and I (one of 60°, the other 45°). The lens *g* then throws the spectrum thus produced on to the slit *f*. The tube K can be rotated round the centre of the iron table (represented in the figure by a circle; it was part of a large Steinheil spectroscope), and thus the slit *f* can be made to cut out portions of any desired wave-length from the spectrum. The eye placed behind the slit then sees the surface of the photometer prism in the colour of the part of the spectrum which is allowed to pass by the slit. Both slits were provided with micrometer screws, the heads of which were divided into 100 parts, and one whole turn of which opened the slit 0·5 mm. The axle round which the telescope, K, turns ended below the table in a small mirror, which turned with the telescope, and which served for reading its position by means of the millimetre scale and telescope, MM. The wave-lengths of the light passing through the slit, *f*, were measured by means of a small spectroscope with graduated arc, the readings of which were converted into wave-lengths by means of a curve drawn from readings of the positions of the principal Fraunhofer lines. The lamp was enclosed in a blackened box, and all the parts of the apparatus covered as much as possible by blackened screens. In the later measurements the lamp was connected with a gas pressure regulator, as variations in the gas pressure are apt to cause changes in the ratio of the brightness of the two beams of light.

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[*] It was made by Messrs. Schmidt and Haensch, of Berlin.

A difficulty which was encountered at first was due to the ends of the glass tubes containing the solutions not being exactly parallel. They act as prisms of very small angle, and deflect the light which passes through them to some extent; so that, after adjusting the two images of the slit, *c*, which are formed by the two lenses, *a* and *b*, to fall together at *f*, and putting in the tube of solution, the two images no longer coincide. The light coming from the lens, *b*, is unchanged, while that from *a* has a colour differing from that which it had at first. It was therefore necessary to have some method of adjusting quickly and accurately the two images to fall together. This was done by allowing the light reflected from the front surface of the prism, G, to fall on a mirror, N, from which it was reflected into the telescope, O. This was an ordinary telescope, in which the two images of the slit, *c*, were seen, side by side, as two bright lines, which could easily be brought to coincidence by rotating somewhat the prism, F. A glance at fig. 2 will show that, by a small rotation of this prism, the direction of the light from the lens, *b*, is unchanged, while that of the light from *a* is changed by twice the angle through which the prism is rotated.

The careful adjustment of the apparatus was also necessary in order to obtain good results. This was done briefly as follows:—

The collimator lenses were placed at their focal distance from the slit, *c*, and the lamp arranged (the ground glass plate being removed) so that they were both just filled with light. The triangular ground glass plate on which the prisms stand was then placed as nearly as possible horizontal, and the prisms adjusted with their refracting edges vertical by reflecting the light from the slit, *c*, first from one surface and then from the other into the telescope, K (the slit of this telescope being replaced by an eye-piece with cross threads) until the images reflected from the two surfaces were seen in the same position. The telescope, K, was then placed horizontal, and the collimator levelled, so that the images of the slit, *c*, seen in the telescope were in the centre of the field of view.

The right-angled prism, E, was then put in position and levelled, so that the light underwent no vertical displacement on passing through it. The Lummer and Brodhun prism is next treated in the same way, and finally the two refracting prisms are put in position and adjusted to minimum deviation. The angle at which the light was internally reflected in the right-angled prism was in my apparatus 43° 30′. As the critical angle for flint glass is 37° 19′ there was no danger of the internal reflexion being incomplete. Finally it was found to be of great importance that the light should pass centrally through the tube of solution. To adjust the tube, cross threads were attached to the lens, *a*. This was conveniently done by means of a brass ring which just slipped over the tube carrying the lens to which the threads were attached. A card with a pin-hole in it was set up at a distance of about a metre and half from the lens in such a position that the prolongation of the line joining the centre of the slit, *c*, and the intersection of the cross threads passed through the pin-hole. The tube of solution, also provided with cross threads at each end, was then placed in position and adjusted, so that, on placing the eye behind the little hole in the card, the intersections of all three cross threads fell together. It was found that a perfectly satisfactory adjustment could be got in this way.

Method of making the Readings.

The sector being open to its full extent, the brightness of the two lights was arranged by putting a smoke-glass plate in the collimator at *d*, so that the comparison light was a little brighter than the other. The width of the slits was taken as small as was consistent with having sufficient light. Five readings were then made of the ratio between the intensities of the two beams. The tube of solution was then put in position, the two images adjusted to coincide, and five more readings made. Then the readings were repeated, the tube being, however, the second time placed in such a position that the deflection of the light was in the opposite direction; consequently the tube rotated 180° round its long axis.

When the means of these sets of readings did not agree, the adjustment was generally defective, and was repeated. As the apparatus was not very rigid, owing to its size, the adjustments had to be repeated every two or three weeks. That of the tube of solution was done before each set of measurements, and the wave-lengths of the light were read after each day's work. The following readings were made on a solution of $CuSO_4$ in the tube 1 meter long; they are readings of average goodness, and will serve to show the sort of differences which occurred between the single readings. The numbers given are the openings of the sector.

Wave-length.	Single readings.	Mean.
621—613·5	59·0, 57·8, 57·2, 58·9, 56·8	57·94 solution out.
	11·8, 11·5, 11·8	11·73 solution in.
	11·2, 11·5, 12·0	11·53
	57·2, 59·1, 58·4, 58·0, 57·9	58·12 solution out.
516—510·5	69·8, 71·3, 73·4, 72·0, 69·9	71·30 solution out.
	61·8, 60·9, 62·5, 57·0, 60·1	60·46 solution in.
	58·9, 59·3, 61·0, 58·0, 63·0	60·04
	72·0, 70·9, 71·0, 71·4, 72·3	71·52 solution out.

Reduction of Results.

The quantity which is directly measured is the amount of light which is lost by passing through a system consisting of two glass plates with a layer of aqueous solution between them. In order to calculate from these numbers the part of the light absorbed by the dissolved substance, it is necessary to allow for the light reflected or absorbed by the glass plates and the water. To obtain the necessary formulæ, I have applied the equations given by Stokes ('Roy. Soc. Proc.,' vol. 11, p. 545) to this special case as follows:—

Call the fraction of the light incident on the glass plate from air, which is reflected $\rho = \left(\frac{\mu - 1}{\mu + 1}\right)^2$, and the same quantity when the light is incident from the liquid on the glass plate $\rho' = \left(\frac{\mu' - 1}{\mu' + 1}\right)^2$. μ and μ' are the refractive indices for air to glass and water to glass respectively. And suppose that a fraction $(1 - a)$ of the light is absorbed by passing once through the glass plate. Then, allowing for the repeated reflections between the two surfaces of the plate, the quantity of light (calling its original intensity $= 1$) which will pass into the liquid is

$$t = \frac{(1-\rho)(1-\rho')a}{1-a^2\rho'\rho},$$

and the part reflected will be

$$r = \rho + \frac{(1-\rho)^2\rho' a^2}{1-a^2\rho\rho'}.$$

If the light is incident on the plate from the liquid instead of from air, the part transmitted will be the same, while that reflected will be

$$r' = \rho' + \frac{(1-\rho')^2\rho a^2}{1-a^2\rho\rho'}.$$

Now consider in the same way the layer of liquid as being a plate, the end surfaces of which have the property of transmitting a fraction t of the light incident upon them, and of reflecting parts r or r', according as the light is incident externally or internally, and which further transmits a part, $b\,c$, of the light which passes through its whole length once. Take b to be the transmission coefficient for the solvent, and c that for the dissolved substance. Then the part of the light transmitted by the whole system is

$$\mathrm{T} = \frac{t^2bc}{1-(bc)^2r'^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ (1).$$

T is the quantity which is directly determined; from it we have to calculate c. b, t, and r' are determined by preliminary experiments. The values of t and r' depend on the refractive indices of the glass plates and of the solutions. The solutions used were all so dilute that no appreciable error is made by using the same values for t and r' as are found for pure water.

Having found the value of c, the extinction coefficient ϵ_1, for the dissolved body, is obtained by means of the relation $-\log c = \epsilon_1$.

The calculation of c from equation (1) may be much simplified in the following way. We may write equation (1):—

$$bc = \frac{\mathrm{T}}{t^2}\left(1-(bc)^2r'^2\right).$$

For the glass plates used the greatest possible value of $(bc)^2r'^2$ was 0·0023. So that approximately $(bc)^2 = \frac{\mathrm{T}^2}{t^4}$, from which

$$c = \frac{\mathrm{T}}{t^2b}\left(1-\frac{\mathrm{T}^2}{t^4}r'^2\right),$$

or

$$\epsilon_1 = -\log c = \log t^2b - \log \mathrm{T} - \log\left(1-\frac{\mathrm{T}^2}{t^4}r'^2\right) \ldots\ (2).$$

Log t^2b only depends on the wave-length of the light used, and is most conveniently obtained from a curve drawn once for all. Also $\frac{r'^2}{t^4}$ is almost independent of the wave-length, and may be regarded as a constant, and the values of $\log\left(1 - \frac{T^2}{t^4} r'^2\right)$ read from a curve in which the values of log T are taken for abscissæ. The wave-lengths and value of log T being then directly determined, the calculation of ϵ_1 is extremely simple.

Transmission Coefficients of the Glass Plates.

The plates were of crown-glass 3·4 mm. thick, and of specific gravity 2·480. Their refractive index for sodium light determined by the method of De Chaulnes was 1·525. The refractive index was assumed to vary with the wave-length in the same way as that of a specimen of crown-glass examined by Fraunhofer.*

Measurements were made of the light transmitted by two of these plates placed one behind the other, with the results contained in the following table. In it T is the light transmitted by the two plates, and *a* the transmission coefficient for 3·4 mm. of the glass :—

Table I.

Wave-lengths.	Mean λ.	T.	*a*.
744 —698·3	721·1	0·8561	1·0040
697·5—673	685·2	0·8578	1·0006
676 —652	664·0	0·8542	0·9999
653 —625·7	639·3	0·8465	0·9993
595·5—574·3	584·9	0·8451	0·9989
551·5—538·5	545·0	0·8476	1·0009
519·0—507	513·0	0·8436	0·9991
483·3—473	478·1	0·8374	0·9959
454·2—446·8	450·5	0·8266	0·9900
433·5—426·4	429·9	0·8248	0·9896
418·0—411·0	414·5	0·8214	0·9879

It will be seen that to a wave-length of about 513 the glass was completely transparent, the loss of light on passing through the plates being entirely accounted for by the reflections at their surfaces. A small absorption of light appears to take place in the violet.

Absorption of Light by Water.

As is well known, the presence of dust in water greatly diminishes its transparency. In order, therefore, to get the water as free as possible from dust, ordinary distilled water was taken and distilled a second time from a metal can with block-tin condenser, the first third of the distillate being rejected. This water, filled immediately into the tube, was almost free from visible dust in a layer 1 m. long. It was just visibly blue. I found afterwards that water filtered through a "Chamberland Pasteur candle" was even more devoid of dust than that which had been freshly distilled, and some measurements were made with such filtered water. There was, however, no noticeable difference in the numbers.

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* Its specific gravity was 2·535 and its refractive index for the line D = 1·529 (see Wüllner, 'Experimental-Physik,' II, p. 157).

The actual measurements of the light transmitted by a tube of water 100·1 cm. long are given in the Appendix Table I. A curve smoothed as little as possible was drawn from these numbers, and the values of T in Table II interpolated from it. From them the values of the extinction coefficient for 1 cm. of water $\left(\epsilon = -\frac{\log b}{100\cdot 1}\right)$ were calculated.

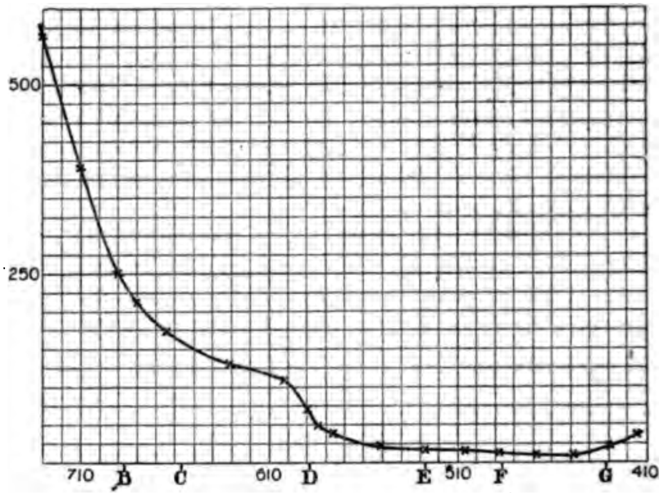
Table II.

Mean λ.	T (from curve).	ϵ.	ϵ (Hüfner and Albrecht).
730	0·246	0·00567	
710	0·37	0·00390	
690	0·51	0·00251	
680	0·5595	0·00210	
664·5	0·6105	0·00173	0·00171
631	0·6732	0·00130	0·00122
602	0·7075	0·00109	0.00109
590	0·765	0·00070	
584	0·814	0·00048	
576·5	0·8315	0·00039	0·00049
551·5	0·861	0·00023	0·00033
527	0·869	0·00019	0·00019
506	0·8728	0·00016	0·00018
487	0·8737	0·00014	0·00016
468	0·8727	0·00012	0·00012
449	0·868	0·00012	0·00012
430	0·8425	0·00023	
415	0·815	0·00035	

The numbers obtained by Hüfner and Albrecht ('Wied. Ann.,' vol. 42, p. 10, 1891) are also included in the table. The agreement is satisfactory except at wave-lengths 576·5 and 551·5. Owing to this lack of agreement I repeated the measurements in this part of the spectrum several times, but never found such large absorptions as Hüfner and Albrecht.

In fig. 3 the values of ϵ are plotted against the wave-lengths. The absorption diminishes very rapidly between the solar lines A and B,

FIG. 3.



Extinction coefficients of water.

and again in the neighbourhood of D, corresponding to the two absorption bands which have been described by several authors.[*] It is also noticeable that the absorption slightly increases in the violet near G.

Solutions of Cupric Salts.

The salts used were cupric sulphate, nitrate, chloride, bromide, and acetate. They were purified carefully, notwithstanding which the solutions made by dissolving them in water were not sufficiently clear to be measured. Filtration through fine Swedish filter paper only improved them a little. The desired freedom from suspended particles was, however, easily obtained by filtration through porous earthenware. The apparatus shown in fig. 4 was found to be very convenient, as it allowed of two or three litres of a solution being filtered without requiring any attention. A and B are ordinary Winchester quart bottles, C is the "Chamberland Pasteur candle." The solution to be filtered is contained in A. B is evacuated, and the apparatus left to itself till the solution has filtered. Two litres could be filtered in an hour, or less if a good vacuum was kept up.

The solutions measured were always analysed as the filtration changes their concentration somewhat. Two methods of analysis were used, and the mean result taken.

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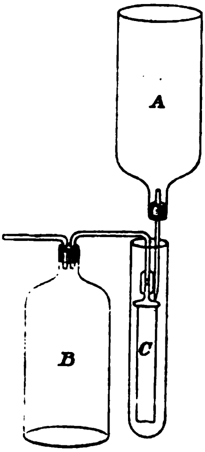
[*] Schönn, 'Pogg. Ann. Ergz.,' 8, p. 670, 1878; Russel and Sapraik, 'Nature,' 22, p. 368, 1880; and Soret and Sarasin, 'Arch. de Sc. Phys. et Nat.,' vol. 11, p. 327, 1884.

FIG. 4.



(1.) From 0·5 to 1 litre of the solution was evaporated to dryness (with a little hydrochloric or acetic acid) on the water bath, the residue dissolved in a small quantity of water, excess of potassium iodide added, and the liberated iodine titrated with sodium thiosulphate solution. In order to obtain accurate results by this method the cupric solution must be concentrated, otherwise the reaction with potassium iodide is incomplete, and the thiosulphate solution must be standardised by means of pure $CuSO_4$ in exactly the same way as it is subsequently to be used in the analyses.

(2.) The solutions, without evaporation, were titrated with potassium cyanide solution in presence of ammonia. Here also the KCN solution was always standardised by means of a solution of $CuSO_4$ of known strength before using it, as its strength changes on keeping. The results of the two methods agreed satisfactorily.

The results of the photometric measurements are contained in Table II Appendix. In order to be able to compare the absorption spectra of the different solutions, the extinction coefficients ϵ for the dissolved substance for 1 cm. layer, and for a concentration of 1 gram molecule per litre, have been calculated from the values of ϵ_1 given by equation 2 (p. 125). The extinction coefficient is proportional to the length of layer (l), and to the concentration (c), so that $\epsilon = \frac{\epsilon_1}{c.l.}$. From these values of ϵ curves were drawn (not smoothed), and from them the numbers contained in the following table (Table III) were interpolated.

The Roman numerals at the head of each column indicate that the figures are derived from the experiment similarly numbered in the Appendix.

Table III.

Mean λ.	ϵ CuSO$_4$.			ϵ CuCl$_2$.	ϵ Cu(NO$_3$)$_2$.			ϵ CuBr$_2$.		ϵ Cu(C$_2$H$_3$O$_2$)$_2$.
	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X and XI.
660	—	—	—	—	—	—	—	—	3·200	
650	2·615	2·939	2·939	2·790	2·730	—	—	—	2·737	
640	2·141	2·362	2·362	2·272	2·282	—	—	—	2·284	
630	1·748	1·892	1·892	1·879	1·862	2·055	1·830	2·034	1·911	4·359
618·5	—	1·305	1·305	1·305	1·253	1·342	1·110	1·385	1·350	3·119
586·8	—	0·623	0·623	0·623	0·587	0·695	0·538	0·640	0·640	1·535
562·9	—	0·310	0·310	0·310	0·279	0·334	0·293	0·291	0·291	0·712
542·8	—	0·158	0·158	0·158	0·110	0·175	0·138	0·158	0·158	0·327
526·2	—	0·080	0·080	0·080	0·048	0·105	0·067	0·083	0·083	0·161
510·3	—	0·045	0·045	0·045	0·020	0·062	0·026	0·039	0·039	0·061
495·3	—	0·021	0·021	0·021	0·005	0·037	—	0·020	0·020	0·035
481·5	—	0·005	0·005	0·005	—	0·019	—	0·009	0·009	0·020

In some cases the numbers given by two different sets of measurements were so nearly the same that they were united to one curve. This was the case with $CuSO_4$ III and II and part of the numbers for $CuCl_2$; and also with the two solutions of $Cu(C_2H_3O_2)_2$ X and XI. Leaving for the present the numbers for $Cu(C_2H_3O_2)_2$ out of account, it is evident that the differences which occur between the numbers obtained with different solutions of the same salt are as great as those which are found between solutions of different salts. The absorption spectra of the four salts $CuSO_4$, $CuCl_2$, $Cu(NO_3)_2$, and $CuBr_2$ must accordingly, in the concentrations used, be regarded as the same.

In the following table (p. 132) the means of the above numbers are compared with the numbers which I formerly obtained ('Phil. Mag.' [5], vol. 33, p. 317, 1892).

These numbers show that the absorptions change very little when the solutions are diluted. The electrolytic dissociation, however, changes considerably. For $CuSO_4$ the fraction of the salt electrolytically dissociated increases from 0·317 in the stronger solution to 0·494 in the more dilute. For $CuCl_2$ the increase is from 0·634 to 0·920. That the change in dissociation produces very little change in the absorption is most simply explained by supposing that the Cu$^{\cdot}$ existing in chemical combination with other atoms in the solution has the same absorption spectrum as the copper ion. In the case of $CuCl_2$ this leads to a result of some interest. For the concentrated aqueous solution of cupric chloride and also its alcoholic and ethereal solu-
ions are brownish-green in colour, and as these solutions doubtless
ontain the molecule $CuCl_2$ we cannot ascribe to it the same absorp-
ion spectrum as to the Cu ion. In the dilute solutions of $CuCl_2$
xamined, therefore, the combined part of the copper probably exists
the form of the ion CuCl. This is also in good agreement with
e observation which I made formerly ('Phil. Mag.,' 1892): that the
oid change in the absorption spectrum which accompanies the dilu-
n of a concentrated solution of $CuCl_2$ ceases when the concentra-
n reaches a value lying between 0·5 and 0·2 gram molecules per litre.
e fractions of the salt dissociated in these concentrations, calcu-
d from the electrical conductivities of the solutions, are 0·41 and
4 respectively. It would appear thus that on diluting a strong
tion of cupric chloride the $CuCl_2$ molecules first dissociate into
ons CuCl and Cl, and that the dissociation of the CuCl ions only
place to any considerable extent after this process is tolerably
lete, that is, after the dissociation of the salt has reached 50 per
This behaviour is very common in the case of dibasic acids, of
a large number have been examined by Ostwald ('Zeitschrift
ys. Chem.,' vol. 3, pp. 186 and 280, 1889).

ill be seen from Table III that the extinction coefficients of

Table IV.

Mean λ.	ε for $CuSO_4$. $c = 0·00373$.	Old numbers. $c = 0·14$.	ε for $CuCl_2$. $c = 0·00397$.	Old numbers. $c = 0·2$.	ε for $Cu(NO_3)_2$. $c = 0·00447$.	Old numbers. $c = 0·175$.	ε for $CuBr_2$. $c = 0·00445$.
660	—	—	—	—	—	—	3·200
650	2·831	—	2·790	—	2·730	—	2·737
640	2·288	—	2·272	—	2·282	—	2·284
630	1·844	—	1·879	—	1·915	—	1·972
613·5	1·305	1·117	1·305	1·042	1·235	1·020	1·367
586·8	0·623	0·586	0·623	0·535	0·607	0·531	0·640
562·9	0·310	0·294	0·310	0·291	0·302	0·328	0·291
542·8	0·158	0·158	0·158	0·152	0·141	0·167	0·158
526·2	0·080	0·091	0·080	0·083	0·073	0·085	0·083
510·3	0·045	0·049	0·045	0·047	0·036	0·043	0·039
495·3	0·021	0·024	0·021	0·026	0·021	0·022	0·020
481·5	0·005	0·014	0·005	0·015	0·009	—	0·009

The concentration, c, is given in gram molecules per litre.

solutions of copper acetate are much larger than those of the other salts. The concentrations of the two solutions, the measurements on which are there given, were 0·003748 and 0·004203 gram molecules per litre respectively. The following numbers (Table V) show that the extinction coefficients diminish rapidly when the concentration becomes smaller. The most dilute solution used still gives larger extinction coefficients than those found for the other copper salts, but it was unfortunately not possible with my apparatus to make accurate measurements with still smaller concentrations, so that I was unable to find out whether the absorption spectrum of copper acetate becomes ultimately identical with that of the others or not; it appears, however, to tend to do so.*

Table V.—Copper Acetate (ε).

Conc.	0·003748	0·0007144	Mean ε for other salts of copper.
λ.	X.	XII.	
650	—	4·412	2·772
640	—	3·625	2·281
630	4·359	2·785	1·902
613·5	3·119	1·450	1·303
586·8	1·535	0·735	0·623
562·9	0·712	0·470	0·303

Two explanations of the abnormal behaviour of copper acetate solutions offer themselves: (1) The electrolytic dissociation into Cu ions may be incomplete, and the still undissociated part of the salt may have a different colour from the Cu ion; (2) the salt may be partially hydrolysed, acetic acid being a so much weaker acid than the others used. In the latter case, however, one would expect the difference between the spectrum of copper acetate and that of the other salts to become greater with increasing dilution, as in the more dilute solutions the hydrolytic decomposition would be most advanced. The opposite, however, is the case. The addition of acetic acid to the solutions should, by diminishing the amount of hydrolysis, bring the copper acetate spectrum nearer to that of the others, if the difference were due to hydrolytic decomposition. The following measurements of ϵ show that again the opposite to this really occurs.

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* The uncertainty of observations of absorption spectra made without the help of photometric measurements is illustrated by the following remark of Knoblauch ('Wied. Ann.,' vol. 43, p. 754). He says: "Ebensowenig wie bei Kupfersulfat zeigte sich bei Kupferacetat mit der Verdünnung eine Veränderung im Character des Spectrums. Die Beobachtung der Absorptions-Grenze im Roth war auch bei diesem Salze für verdünnte Lösungen durch die Absorption des Wassers beeinträchtigt." Table V shows that a not very considerable dilution (about one to five) reduced the absorbing power of a copper acetate solution by about one-half.

Table VI.

Conc.					
$Cu(C_2H_3O_2)_2$	0·0007144	0·0007395	0·00289	0·00479	0·004203
Acetic acid.	—	0·00155	0·0088	0·136	—
λ.	XII.	XV.	XIII.	XIV.	XI.
650	4·412	4·515			
640	3·625	3·730			
630	2·785	2·931	—	—	4·359
613·5	1·450	1·745	2·720	3·321	3·119
586·8	0·735	0·818	1·287	1·760	1·535
562·9	0·470	—	0·681	0·958	0·712
542·8	—	—	—	0·470	0·327

That some very small amount of hydrolytic dissociation takes place is, however, probable. Dibbits* found that a solution of copper acetate in the cold is very weakly acid towards litmus, and that on warming it basic salts separate out, and on boiling the decomposition into acetic acid and basic salt reaches about 50 per cent. The most dilute solutions which I used did not deposit any visible quantity of copper hydroxide on the porous vessel through which they were filtered, and, even after standing for three days, the filtered solution remained perfectly clear. The behaviour of the solutions of copper acetate is, on the other hand, qualitatively at any rate, in agreement with the hypothesis that the substance is dissociated into ions. Professor J. H. van't Hoff was so kind as to place the results of an unpublished set of determinations made by him of the electrical conductivity of solutions of copper acetate at my disposal, for which I take this opportunity of again expressing my indebtedness to him. The numbers are given in Table VII.

μ_∞ for copper acetate is calculated from Kohlrausch's numbers as follows:—μ_∞ for $\frac{1}{2}CuSO_4$ at 14°·1 = 100·2.

The difference between $\mu_\infty\frac{1}{2}(K_2SO_4)$ and $\mu_\infty KC_2H_3O_2$ at 14°·1 = 30·4,

So that we get $\mu_\infty\frac{1}{2}Cu(C_2H_3O_2)_2$ at 14°·1 = 69·8.

It will be noticed that k remains constant till 0·35 of the salt is dissociated, after which it diminishes. This behaviour reminds one strongly of that of the dibasic acids studied by Ostwald ('Zeitschrift für Phys. Chem.,' vol. 3, p. 170, 1889); the only difference is that with the acids k finally increases instead of diminishing. It

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[*] 'Maandblad voor Natuurwetenschappen,' vol. 3, p. 80, 1873, and vol. 4, p. 104, 1874.

Table VII.—Electrical Conductivity of Copper Acetate Solutions. At 14·1°.

V.	μ.	a.	k.
4·06	19·8	0·1418	0·0058
$4{\cdot}06 \times 2$	26·9	0·1927	0·0057
$4{\cdot}06 \times 2^2$	37·1	0·2658	0·0059
$4{\cdot}06 \times 2^3$	48·1	0·3446	0·0057
$4{\cdot}06 \times 2^4$	60·7	0·4348	0·0052
$4{\cdot}06 \times 2^5$	74·7	0·5351	0·0047
$4{\cdot}06 \times 2^6$	90	0·6447	0·0045
$4{\cdot}06 \times 2^7$	101·3	0·7257	0·0037

$\mu_\infty = 139{\cdot}6$.

V = vol. in litres in which 1 gram molecule $Cu(C_2H_3O_2)_2$ is dissolved.

μ = molecular conductivity.

$k = \dfrac{a^2}{(1-a)V}$.

$a = \dfrac{\mu}{\mu_\infty}$ = fraction of the copper acetate dissociated into ions.

seems probable then that just as the dibasic acid first split off one hydrogen atom as an ion and then the second, so the copper acetate splits up first into the ions $Cu(C_2H_3O_2)$ and $(C_2H_3O_2)$, and then the ion $Cu(C_2H_3O_2)$ breaks up, after the first process is fairly complete, into the ions Cu and $(C_2H_3O_2)$. Calculated from the numbers for the electrolytic conductivity given above the quantities of the salt dissociated in the two solutions in Table V are—

Conc. .... 0·003748 = 65 per cent.
0·0007144 = 85 „ (extrapolated).

These two solutions will accordingly contain the ions Cu and $Cu(C_2H_3O_2)$, the greater the quantity of the latter the more will the colour of the solution differ from the normal. The addition of acetic acid to the solution diminishes the dissociation of the copper salt, and so increases the difference of the colour from the normal.

Salts of Dinitrophenol, $C_6H_3(NO_2)_2OH$ (2.4·1).

The dinitrophenol used was the ordinary one; the specimen obtained from Kahlbaum melted at 114—115°. The potassium and ammonium salts were prepared from it. They both crystallise very well, the former in orange, the latter in bright yellow needles, and are therefore easily obtained pure.

The potassium salt lost on prolonged heating to 100° .... 3·95 per cent. H_2O.
Calculated for $C_6H_3(NO_2)_2OK + \frac{1}{2}H_2O$ ...... 3·9 „

The ammonium salt volatilises on heating. The ammonia was therefore determined.

NH_3 found .... 7·69 per cent.
NH_3 calculated for $C_6H_3(NO_2)_2ONH_4$ ...... 7·76 „

As I could find no satisfactory way of analysing the dilute solutions of these salts, I made them by dissolving a weighed quantity of the carefully purified salt in water. The solutions so obtained were fairly clear. The values of ϵ (the extinction coefficient for the dissolved body for 1 cm. layer and 1 gram molecule per litre concentration) are given in the following table. The experimental numbers are given in the same way as before in the Appendix Table III.

The numbers given in Table VIII are obtained from the numbers in Table III of the Appendix by graphical interpolation. The concentrations are in gram molecules per litre.

The two most concentrated solutions used give somewhat smaller numbers than the others, which may probably be attributed to the electrolytic dissociation being incomplete in the stronger solutions. The numbers obtained with the other solutions vary irregularly; by taking their mean the following numbers are obtained.

λ.	K salt.	NH_4 salt.
530	—	2·195
520	4·19	3·995
510	10·82	10·85
500	26·26	25·1
490	67·23	74·5
480	194·3	173·4
470	398·5	368·0

The absorption increases so extremely rapidly between the wave-lengths 470 and 500 that a small error in the measurement of the wave-length can make a great difference in the value of ϵ obtained. Taking this into account the absorption spectra of the potassium and ammonium salts may be regarded as the same. These salts dissociate electrolytically into the ions K^+ or NH_4^+ and $C_6H_3(NO_2)_2O-$. The mean of the above numbers may therefore be taken as representing the absorption spectrum of the ion $C_6H_3(NO_2)_2O$.

Dinitrophenol.

The behaviour of the solutions of the free dinitrophenol is of some interest. The solutions in benzene or ether are brownish-yellow, but the colour is faint, and unless they are fairly concentrated they appear colourless. An alcoholic solution is much more strongly

Table VIII.

	Potassium salt of dinitrophenol.					Ammonium salt of dinitrophenol.			
c =	0·001646	0·0004107	0·0001027	0·0001027	0·00009861	0·004739	0·001185	0·001185	0·0004027
λ	XVI.	XVII.	XIX.	XVIII.	XX.	XXI.	XXII.	XXIII.	XXIV.
670	0·121								
630	0·270								
590	0·181								
560	0·305	—	—	—	—	0·319			
550	0·488	—	—	—	—	0·409	0·939		
540	0·823	—	—	—	—	0·715	1·278	1·48	
530	1·995	—	—	—	—	1·614	2·15	2·04	
520	—	4·19	—	—	—	—	4·25	3·74	
510	—	10·85	—	—	10·8	—	—	10·85	
500	—	25·01	26·8	—	27·0	—	—	25·1	
490	—	—	62·1	76·1	63·5	—	—	—	74·5
482·5	—	—	124·94	151·8	136·5	—			
480	—	—	—	194·3	—	—	—	—	173·4
470	—	—	—	398·5	—	—	—	—	368·0

yellow, and a saturated aqueous solution is bright yellow. A drop of potash solution added to either the alcoholic or aqueous solutions increases the depth of colour. The addition of hydrochloric acid diminishes it, till, with some excess of acid, it disappears almost entirely. The fact that the solutions of dinitrophenol in benzene or ether are only faintly coloured, suggests that the undissociated dinitrophenol molecule is devoid of any great absorbing power in the visible spectrum. The action of the hydrochloric acid in decolorising the solutions of dinitrophenol is also due to its converting the dissociated molecules into undissociated molecules.* In order to obtain the spectrum of the undissociated dinitrophenol a solution of dinitrophenol to which excess of hydrochloric acid had been added was used.

The dinitrophenol had been recrystallised from water, notwithstanding which the solutions obtained from it were never sufficiently clear. They had, therefore, to be filtered through the porous earthenware and analysed. The analysis was made by titration with a dilute ammonia solution, using litmus as indicator. The change of colour not being very sharp, standard hydrochloric acid was added till the liquid was acid, and then ammonia till alkaline, and so on several times. Tests with solutions of known concentration showed that accurate results could be obtained in this way. The following table contains the extinction coefficients of the solution containing excess of hydrochloric acid, and also for comparison, the mean of the numbers obtained with the potassium and ammonium salts.

Table IX.

	$c = \begin{cases} 0{\cdot}001202 \text{ dinitrophenol} \\ + 0{\cdot}2132 \text{ gr.mol. HCl.} \end{cases}$	Mean ϵ for K and NH_4 salts.
λ.		
520	0·321	4·09
510	0·355	10·83
500	0·397	25·68
490	0·456	70·86
480	0·553	183·85
470	0·600	383·25
460	0·974	
450	2·284	

It is evident that the absorption of the acid solution is almost negligible compared with that of the solutions of the salts.

With the help of these numbers it is possible to calculate the fraction of the dinitrophenol which is electrolytically dissociated in a solution, a measurement of its extinction coefficient for any one wave-length having been made. If x is the part which is dissociated into its ions, the extinction coefficient of the solution (calculated for unit concentration and 1 cm. layer) will be

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* Arrhenius, 'Zeitschrift Phys. Chem.,' vol. 2, p. 284, 1888, and vol. 5, p. 1, 1890.

$$\epsilon_{(\text{sol.})} = x\epsilon_{(\text{ion})} + (1-x)\,\epsilon_{(\text{undiss.})}.$$

Table X contains the results of the measurements of a number of solutions of dinitrophenol of different concentrations. For the experimental details see Appendix, Table V. In Table X, ϵ_s, ϵ_i, and ϵ_u, are the extinction coefficients for the solution actually measured, for the ion, $C_6H_3(NO_2)_2O$, and for the undissociated molecule of dinitrophenol respectively. The values of ϵ_i, as remarked above, are taken as the mean of the measurements made with dilute solutions of the potassium and ammonium salts.

x the fraction dissociated is calculated from the equation given above.

Table X.

λ.	c = concentration.	ϵ_s.	ϵ_i.	ϵ_u.	x.	Mean x.	x (cal.).
480·2	0·001581	46·69	181·2	0·550	0·2554		
496	—	9·44	36·23	0·419	0·2519		
497·5	—	7·829	32·47	0·410	0·2314		
497·7	—	6·421	31·76	0·409	0·1915		
487·2	—	20·8	91·1	0·480	0·2242	0·2309	0·2010
479·2	0·0007905	61·58	198·5	0·564	0·3083		
493·3	—	11·52	46·2	0·432	0·2423		
480·8	—	36·93	172·2	0·544	0·2120	0·2509	0·2715
493·6	0·000395	17·11	44·84	0·431	0·3756		
476·8	—	83·07	241·8	0·597	0·3419	0·3588	0·3600
472·1	0·0001581	163·9	332·3	0·663	0·4923		
493·2	—	23·83	46·7	0·433	0·4017	0·4470	0·5018
496·2	0·00001581	32·3	35·67	0·418	0·9044		
473·8	—	278·5	298·5	0·641	0·9329	0·9187	0·8539
480·1	0·002886	32·86	182·2	0·551	0·1779		
479	—	33·37	202	0·568	0·1629	0·1709	0·1532

The numbers under x calculated are the fractions of the substance electrolytically dissociated calculated from the determinations of the electrical conductivity of solutions of dinitrophenol made by Bader. ('Zeit. Phys. Chem.,' vol. 6, p. 298, 1890). He found the dissociation constant of dinitrophenol K = 0·008. This is connected with x by the formula $K = \dfrac{100x^2}{(1-x)V}$.

V, the volume in litres in which 1 gram molecule is dissolved, may be replaced by $\dfrac{1}{c}$, which gives

$$x = \frac{1}{200c}\left[\sqrt{3{\cdot}2c + 0{\cdot}000064} - 0{\cdot}008\right].$$

The agreement found between the numbers obtained from two so very different properties as the absorption of light and electric conductivity, is so striking, taking into account the very considerable experimental error, that little doubt can be felt as to the substantial accuracy of the hypothesis on which the whole calculation is based.

Ferric Salts.

The changes of colour which take place when the aqueous solution of a ferric salt is diluted are so considerable that they appear to offer a very suitable means of determining the amount and nature of the decomposition which the salt undergoes.

In 1856, Péan de St. Gilles,[*] made a study of the decomposition which ferric salts undergo on heating their aqueous solutions. He noticed the peculiar appearance of the solutions containing ferric hydroxide, they being clear by transmitted, but apparently turbid by reflected light. He was aware that the hydrate exists in the liquid in a state of pseudo solution and uncombined with the acid present, as the following remark shows :—"Il semblerait plutôt que l'acide acétique exerce sur l'hydrate une action purement mécanique en divisant à l'extrême les particules insolubles et simulant ainsi une dissolution véritable."

In 1861, Graham[†] showed how the colloidal ferric hydroxide can be prepared by dialysis, and described its principal properties. He regarded it as being different from the soluble hydroxide obtained by Péan de St. Gilles, by heating the solution of ferric acetate.

Gunning,[‡] in 1869, Tichborne,[§] in 1871, and Berthelot,[||] in 1873, made studies of the decompositions which occur both on heating the solutions and on diluting them with water. Their results, however, do not lend themselves to a calculation of the amount of decomposition which has taken place. In 1871, Krecke[¶] made a number of determinations of the quantity of ferric hydroxide formed in solutions of ferric chloride of different concentrations, and at various temperatures. The amount of ferric hydroxide present was determined by precipitating it with sodium chloride. He found that the precipitate formed by heating solutions of ferric chloride containing more than 4 per cent. of the salt contained a variable amount of chlorine. The more dilute solutions deposit only ferric hydroxide. He found also that the amount of decomposition increases with rising temperature and increasing dilution.

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[*] 'Ann. Chim. Phys.,' (3), vol. 46, p. 47, 1856.

[†] 'Phil. Trans.,' 1861, p. 209.

[‡] 'Aanteekeningen, etc., Prov. Utrechtsche Genootschap, 1869, p. 14.

[§] 'Roy. Irish Acad. Proc.,' 1871, p. 169.

[||] 'Ann. Chim. Phys.,' (4), vol. 30, p. 145, 1873.

[¶] 'J. prakt. Chem,' (2), vol. 3, p. 286, 1871.

Finally, G. Wiedemann,* in 1878, published an investigation on the hydrolysis of ferric salts, the determinations of the amount of hydrolysis which had taken place being made by means of the molecular magnetism of the solutions. The molecular magnetism of iron in the form of hydroxide being 0·16 of its value in a strongly acid solution, a measurement of the magnetism of a solution of known strength allows the ratio in which the iron is divided between the two forms to be calculated.

The fact that, as Krecke observed, the precipitate formed by heating solutions of ferric chloride contains chlorine, and also an observation made by Picton and Linder,† that on filtering a solution of colloid ferric hydroxide containing chlorine through a porous cell the filtrate contained neither iron nor chlorine, led me in the first place to attempt to decide whether the hydroxide formed in dilute solutions of ferric chloride contained chlorine or not.

For this purpose solutions of known concentration were filtered through a porous cell. The cell was first well washed with distilled water, and then a quantity of the solution filtered through it to remove the water from its pores. The next portion of the filtered solution was collected and analysed. The difference between the composition of the original solution and that of the filtrate gives the composition of the substance held back by the porous cell. The filtration apparently removed the hydroxide completely from the solution. The yellow-brown coloured solutions were, after filtration, either completely colourless or very faintly yellow, according to their concentration. This method of analysis has the drawback that the solution changes its concentration to some extent during the filtration partly owing to evaporation in the vacuous vessel into which it is filtered, partly owing to the difficulty in removing the water which has served for washing the cell from its pores. A direct analysis of the precipitate is hardly possible as it cannot be collected on a filter, nor can it be washed by decantation, as it refuses to settle. Further, to wash it with water is not allowable, as it would tend to change its composition. The ferric chloride used was purified by repeated recrystallisation of the hydrate, $Fe_2Cl_6 12H_2O$;‡ an analysis of its solution showed that it contained iron and chlorine in the ratio represented by its formula.

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* 'Wied. Ann.,' vol. 5, p. 45, 1878.

† 'Journ. Chem. Soc.,' 1892, p. 152.

‡ Bakhuis Roozeboom, 'Zeit. Phys. Chem.,' vol. 10, p. 477, 1892.

250 c.c. of a solution of the salt gave $\begin{cases} 0\cdot1042 \text{ gram } Fe_2O_3. \\ 0\cdot5639 \quad " \quad AgCl. \end{cases}$

From which ratio $\frac{Cl}{Fe} = 1\cdot913$.

Ratio calculated for $Fe_2Cl_6 = 1\cdot903$.

Two other solutions which were subsequently analysed, contained

1·882 grams eqs. Fe per litre to 1·864 grams eqs. Cl.
and 0·0303 „ „ 0·0296 „

The dilute solutions made by diluting a concentrated solution of known concentration, were allowed to stand for some weeks at the ordinary temperature (15—16°), so that a condition of equilibrium was almost certainly reached.

The following table contains the results of the filtration experiments. The concentrations are here (as elsewhere in the paper) given in gram molecules per litre, the molecule of the ferric salts being always taken, for convenience, as the quantity containing one atom of iron.

Table XI.

Concentration of unfiltered solution.		Concentration of filtered solution.		Loss per cent.		k_1.	k_2.
Fe.	Cl.	Fe.	Cl.	Fe.	Cl.		
0·00505	0·0148	0·00319	0·01453	36·83	1·93	922	346
0·002525	0·0074	0·00126	0·00728	50·10	1·75	787	394
0·001263	0·0037	0·000445	0·00376	64·77	−1·48	664	430
0·000505	0·00148	0·000095	0·00143	81·19	3·51	565	459

The decrease in the concentration of chlorine in the solutions, caused by filtration, is so small (in one case there was even an increase), that it appears probable that the substance separated from the solutions is really a hydroxide of iron, and contains no chlorine. The changes of concentration of the chlorine are probably sufficiently explained by the errors inherent in the method used, which have been already mentioned. On the other hand the loss of iron increases as the solutions become more dilute.

Walker* found, in an investigation on the hydrolysis of the salts of some weak bases, that the decomposition took place in agreement with the law of Guldberg and Waage. This is, however, here not the case. The equilibrium which takes place in dilute solutions of ferric chloride may be most simply represented by the equation

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* 'Zeitschrift f. Phys. Chem.,' vol. 4, p. 319, 1889.

$$FeCl_3+3H_2O \rightleftarrows Fe(OH)_3+3HCl.$$

Suppose that to begin with one gram molecule of $FeCl_3$ was dissolved in n molecules of water, neither hydrochloric acid nor ferric hydroxide being present. When the equilibrium is reached, suppose that a fraction x of the ferric chloride molecules have been decomposed. Then we have $(1-x)$ molecules $FeCl_3$, x molecules $Fe(OH)_3$, $3x$ molecules HCl, and $(n-3x)$ molecules of water. As n is large compared with $3x$, this may be taken as the same as n.

According to Guldberg and Waage these quantities should be connected together by the equation

$$\frac{(1-x)\,n}{x^2} = 3\mathrm{K} \quad\ldots\ldots\ldots\ldots\ldots\ldots \quad (3).$$

x is identical with the loss of iron given in Table XI divided by 100, and, calling the concentration of the original unfiltered solution (with regard to Fe) $= c$, we have 1 gram molecule $FeCl_3$ in $1/c$ litres, or, as 1 litre of water contains 55·5 gram molecules, we have 1 gram molecule $FeCl_3$ in $55{\cdot}5/c = n$ gram molecules H_2O. Putting these values in the above equation we get

$$\frac{(1-x)}{x^2\,.\,c} = \frac{3}{55{\cdot}5}\mathrm{K} = k_1.$$

The values of k_1, calculated from the different values of x, are given in Table XI, and are evidently very far from being constant, as is required by the law of Guldberg and Waage.

The agreement is hardly improved by regarding the ferric hydroxide as an insoluble body and putting its active mass constant. The equation then becomes

$$\frac{(1-x)}{xc} = \text{const.} = k_2.$$

The values of k_2 also vary considerably, as Table XI shows.

According to the equation for the equilibrium of four electrolytes given by Arrhenius,* however, K, on the right-hand side of equation (3), is not a constant. For Arrhenius regards only those parts of the electrolytes which are dissociated into their ions as the active masses, so that if α_1, α_2, α_3, and α_4 are the fractions of the electrolytes $FeCl_3$, H_2O, $Fe(OH)_3$, and HCl respectively, which are dissociated, we must substitute for the equation of Guldberg and Waage the expression

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* 'Bihang k. Svensk. Vet.-Ak. Hand.,' vol. 8, No. 14, 1884.

$$(1-x)\,\alpha_1 \times \frac{n}{3}\,\alpha_2 = x\alpha_3 \times x\alpha_4.$$

As Arrhenius* has shown, the values of α_1, α_2, &c., are to be obtained from equations of the form

$$\frac{\alpha_1 m}{n(1-\alpha_1)} = \mathrm{K}_1 \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (5).$$

m is the whole number of dissociated molecules of all kinds in the solution, and K_1, K_2, &c., are the characteristic dissociation constants for the four electrolytes. The values of α_2 and α_3 are very small, so that we have

$$\alpha_2 = \mathrm{K}_2\frac{n}{m} \qquad \text{and} \qquad \alpha_3 = \mathrm{K}_3\frac{n}{m}.$$

α_4, the fraction of the hydrochloric acid dissociated, may be put $= 1$, so that the equation becomes, putting $n = 55{\cdot}5/c$,

$$\frac{(1-x)}{x^2c} = \frac{3}{55{\cdot}5}\,\frac{\mathrm{K}_3}{\mathrm{K}_2}\,\frac{1}{\alpha_1} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (6).$$

Neglecting small quantities, we have

$$m = (1-x)\,\alpha_1 + x.$$

Substituting which in (5),

$$\frac{\alpha_1\left((1-x)\,\alpha_1 + x\right)}{1-\alpha_1} = n\mathrm{K}_1 \quad \ldots\ldots\ldots\ldots\ldots \quad (7).$$

The values of α_1, the dissociation coefficients of ferric chloride, are unknown. I have attempted to test the agreement of the experiments with the Equations 6 and 7, thus:—Each experiment gives a value for $\frac{3}{55{\cdot}5} \cdot \frac{\mathrm{K}_3}{\mathrm{K}_2} \cdot \frac{1}{\alpha_1} = \frac{1}{b}$ (say), or, writing the constant part $\frac{3}{55{\cdot}5} \cdot \frac{\mathrm{K}_3}{\mathrm{K}_2} = \frac{1}{d}$, we get $\alpha_1 = \frac{b}{d}$.

Substituting this in Equation 7,

$$\frac{cb^2}{d-b} + cdx = 55{\cdot}5\,\mathrm{K}_1 d.$$

From these equations d and K_1 can be found. The following numbers were obtained:—

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* 'Zeit. Phys. Chem.,' vol. 5, p. 1, 1890. *Cf.* J. J. van Laar, 'Die Thermodynamik in der Chemie,' 1893, where a convenient collection of the formulæ is to be found.

$$\left.\begin{array}{r} d = 0{\cdot}00163 \\ 0{\cdot}00196 \\ 0{\cdot}00205 \end{array}\right\} \qquad \begin{array}{r} 55{\cdot}5\,K_1 d = 0{\cdot}0000085 \\ 0{\cdot}0000072 \\ 0{\cdot}0000069 \\ 0{\cdot}0000075 \end{array}$$

Mean used = 0·002

Mean = 0·0000075

The numbers now found are approximately constant. As $d = 18{\cdot}5\,\frac{K_2}{K_3}$, we get $\frac{K_3}{K_2} = 9250$.

That is, the dissociation constant of ferric hydroxide is about 9000 times that of water. Ferric hydroxide is thus an extremely weak base. From $55\ K_1 d = 0{\cdot}0000075$ we get also $K_1 = 0{\cdot}000068$. For comparison,* the dissociation constants of KCl = 0·00958, that of $CuSO_4$ = 0·0001898, so that ferric chloride must be regarded as one of the exceptionally slightly dissociated salts.

A number of photometric measurements were made of the absorption spectra of solutions of ferric chloride, but their peculiar properties make it rather difficult to obtain accurate results. The solutions were made by diluting a concentrated solution of ferric chloride, which had been made as clear as possible by filtration through paper. They were then allowed to stand till no further change took place in their colour; if they were then without visible turbidity, they were used for measurement, but, owing to the peculiar fluorescent appearance of solutions containing colloid ferric hydroxide, it was not easy to see whether they were really free from solid suspended particles or not.

The numbers which are contained in Table VI of the Appendix, and from which the following table is calculated, give results which agree approximately with the results of the filtration experiments given above. The calculation of the amount of ferric hydroxide in the solutions was made as follows:—The most dilute solution of $FeCl_3$ which was used (XXV, Table XII), and another one to which a small quantity of ammonia had been added (XXVI, Table XII), gave nearly the same numbers. The decomposition in these solutions appeared to be complete, as neither of them gave a red colour on addition of excess of ammonium sulphocyanide. Solutions of colloid ferric hydroxide, obtained by dialysis, also give no red colour with sulphocyanide. I took the absorptions of these solutions as corresponding to complete hydrolysis. The smaller absorptions of the other solutions will then correspond to a proportionately smaller decomposition, if we assume that the whole absorption is due to the ferric hydroxide, an assumption which is quite allowable, as the filtration

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* The numbers are from van Laar, *loc. cit.*, p. 154. To convert them into the units used by Ostwald they must be divided by 0·018.

Table XII.

c = 0·00002525		0.000332 + 0·000407NH_3.	Mean of XXV and XXVI.	0·000101		0·000101		0·00036		0·000505	
				ε.	Per cent. hydroxide.	ε.	Per cent. hydroxide.	ε.	Per cent. hydroxide.	ε.	Per cent. hydroxide.
λ	XXV.	XXVI.		XXVII.		XXVIII.		XXIX.		XXX.	
580	39·3	—	39·3	31·8	80·9						
560	57·3	55·9	56·6	47·0	83·0	—	—	49·2	86·9	36·9	65·2
540	84·8	85·2	85.0	75·6	86·9	—	—	68·6	80.7	72·1	84·8
520	124·0	131·4	127·7	115·0	90·1	117·6	92·1	112·3	88·0	113·1	88·8
500	168·0	195·5	181·7	152·4	83·9	143·8	79·2	157·6	86·8	167·0	91·9
480	221·7	—	222·0	203·3	91·7	180·3	81·3				
460	308·1	—	308·0	—	—	183·3	91·6				
Mean per cent. decomposed =					86·1	—	86·0	—	85·6	—	82·7
Per cent. decomposed, by interpolation from filtration experiments						95·0		—	86·0	—	81·0

experiments show, the removal of the hydroxide from the solutions leaving them nearly colourless.

All the solutions in which the hydrolysis was incomplete gave the red colour with ammonium sulphocyanide.

The measurements which G. Wiedemann made of the amount of hydrolysis in the more concentrated solutions of ferric chloride appear to be in harmony with the results given in Table XI. The numbers, when plotted against concentrations, give two curves, which appear to be parts of the same curve.

Colloid Ferric Hydroxide.—A solution was prepared by dialysing a solution of ferric chloride in which a quantity of ferric hydroxide had been dissolved. It was clear red-brown by transmitted but apparently turbid by reflected light; it gave no deposit on standing for three months, and passed through filter paper without leaving any residue. A solution of NH_4SCN coagulated it, but gave no red colour. Analysis showed that it contained 12·4 per cent. of the amount of chlorine corresponding to the formula $FeCl_3$. The extinction coefficients obtained for different solutions, made by diluting this solution, are given in Table XIII; they are obtained by graphic interpolation from the experimental numbers given in the Appendix, Table VII.

Table XIII.

c =	0·09855	0·02531	0·000568	0·000568	0·0001254	Mean for dil. $FeCl_3$ solutions.
λ.						
660	6·52	5·94				
640	7·98	7·46				
620	9·91	9·40				
600	12·61	12·02	12·35			
580	16·85	16·38	17·20	—	—	39·3
560	26·64	25·42	26·45	—	30·5	56·6
540	44·95	43·50	46·30	46·5	51·2	85·0
520	—	—	80·10	82·9	84·4	127·7
500	—	—	—	136·9	139·2	181·7
480	—	—	—	197·4	200·9	221·7
460	—	—	—	218·0	219·7	308·1
440	—	—	—	285·2	277·5	
430	—	—	—	—	354·0	

The numbers, on the whole, show some tendency to increase as the dilution increases. A comparison with the numbers obtained for the completely hydrolysed solutions of ferric chloride (last column, Table XIII) shows that the spectra are entirely different. There can be little doubt that both the solutions contain colloid ferric hydroxide. The filtration experiments show that this is the case in the ferric chloride solution, and one would imagine that if all the iron exists as hydroxide in a solution containing an equivalent quantity of chlorine this should be certainly the case in one containing only some 12 per cent. of that quantity of chlorine. An explanation is, I think, suggested by an observation made by Picton.* He noticed that a solution of antimony hydrosulphide, obtained by pouring a solution of tartar emetic into one of sulphuretted hydrogen, possessed only a faint fluorescence, and, under the microscope, no particles were visible in it. On dialysing the solution the fluorescence increased, and gradually minute particles became visible under the microscope; finally the solution coagulated. Picton and Linder ('J. Chem. Soc.,' 1892, 154) found also that silicic acid behaves in quite a similar way. In a colloid solution of silicic acid containing hydrochloric acid the particles were too small to reflect light, while after dialysis they were large enough to do so. Krecke (*loc. cit.*) also found that, on dialysing a concentrated ferric chloride solution, it finally coagulated. It would appear as though the dialysis always favoured the formation of larger molecular aggregates. A possible reason for this, in the case of ferric hydroxide, at any rate, is not difficult to see. In the solutions containing hydrochloric acid the hydroxide must be regarded as constantly being acted upon by the acid with formation of ferric chloride, which is at once decomposed again. Consequently the hydroxide never has the opportunity of forming large molecular groups.

Solutions containing excess of Hydrochloric Acid.

The addition of a small quantity of hydrochloric acid to a solution containing the hydroxide diminishes the quantity of the latter, and therewith the intensity of the brown colour of the solution, to a marked extent. I found, for example, by filtration through the Chamberland Pasteur candle, that the addition of one-sixth of an equivalent of HCl to a dilute solution of ferric chloride reduced the quantity of $Fe(OH)_3$ present by nearly a half. The following are the numbers:—

Concentration in gram mols. per litre before filtering.		After filtering.		Loss per cent.	
$FeCl_3$.	HCl.	Fe.	Cl.	Fe.	Cl.
0·001263	—	0·000445	0·00376	64·77	−1·48
0·001263	0·000635	0·00084	0·00432	33·49	+0·46

---

* 'Chem. Soc. Journ.,' 1892, p. 142.

Larger quantities of acid seem to prevent the formation of ferric hydroxide altogether, as the acid solutions could all be filtered through the porous cell without leaving any deposit. The analyses also showed that their concentration was not diminished by the filtration.

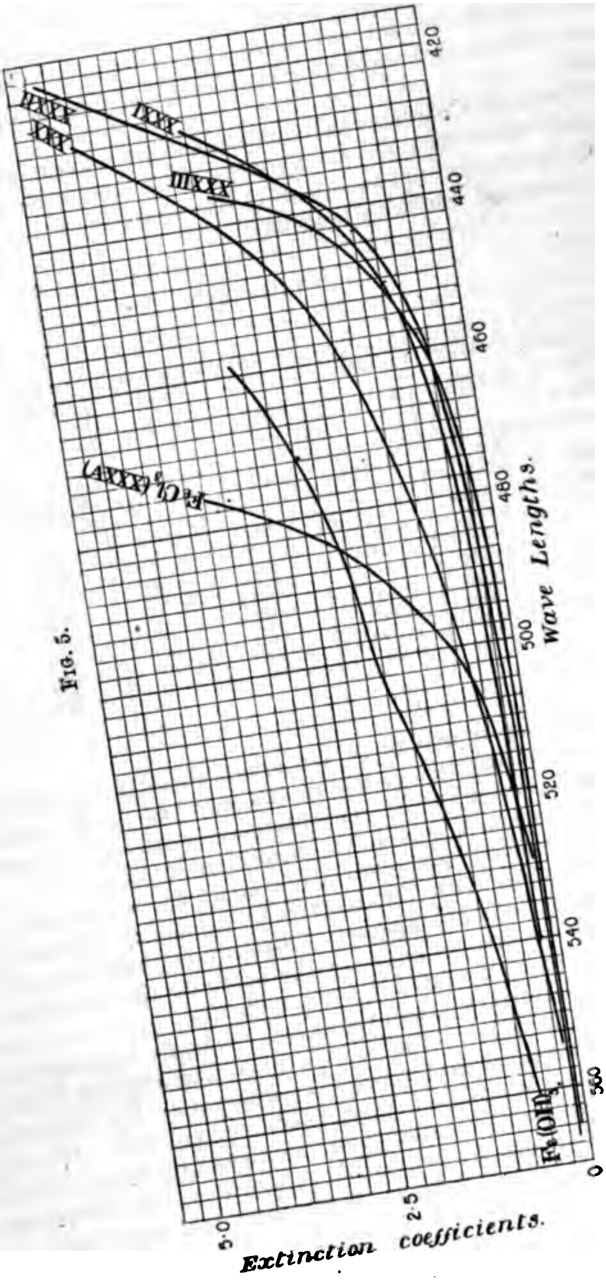
Table XIV contains the numbers which were obtained with a series of acid solutions. The arrangement of the table is the same as before, only the number of equivalents of free hydrochloric acid added to one equivalent of iron are also given.

Table XIV.

Conc. Fe	0·001995	0·001284	0·001279	0·00425	0·0013	0·3095
HCl	0·00466	0·0094	0·0178	0·06	0·035	0·814
$\frac{\text{Eq. acid..}}{\text{Eq. Fe...}}$	0·7786	2·44	4·64	4·706	8·97	0·8767
λ.	XXX.	XXXI.	XXXII	XXXIII.	XXXIV.	XXXV.
580	0·098					
560	0·139	—	0·122	0·083	0·061	0·117
540	0·265	0·233	0·115	0·084	0·080	0·135
520	0·419	0·242	0·130	0·082	0·112	0·290
500	0·661	0·289	0·182	0·099	0·157	0·775
480	1·043	0·424	0·298	0·178	0·219	2·950
460	1·682	0·601	0·777	0·575	0·493	11·01
440	3·175	1·643	1·495	2·100	1·555	
430	4·340	2·83	—	—	3·375	

These numbers appear to be somewhat irregular, but a glance at fig. 5 will show that this is due to the way the curves cross each other. The curve XXX of the solution containing the smallest excess of hydrochloric acid has a strong resemblance to the curve for $Fe(OH)_3$, which is included in the figure for comparison (with ordinates divided by 100). If the whole absorption of solution XXX were due to hydroxide, the amount of hydrolysis would only be ½ to ⅓ per cent. of the salt present. In the curve for the next solution, XXXI, the similarity to that of the hydroxide has disappeared, and as the quantity of acid is increased the curves evidently approach more and more towards that of the concentrated acid solution XXXV, which may be taken as representing approximately the absorption spectrum of the undecomposed molecule of ferric chloride. Whether these changes are due to the change of the dissociated ferric chloride into undissociated, or to the conversion of a soluble oxychloride into ferric chloride cannot be decided. It is certain that these solutions contain none of the colloid hydroxide, but they possibly contain a soluble oxychloride. Wiedemann found that solutions of ferric nitrate and sulphate, containing excess of acid, still contain a con-

Fig. 5.


XXXII
XXXI
XXX
XXXIII
Fe_2Cl_6 (XXXV)
$Fe_2(OH)_6$
420
440
460
480
500
520
540
560
Wave Lengths.
5·0
2·5
0
Extinction coefficients.

derable quantity of hydroxide. He has given no measurements
id solutions of the chloride, but as I found that the acid soluti
ferric nitrate and sulphate behave very much in the same way
e chloride with respect to colour, we may perhaps conclude th
l these solutions contain some really soluble substance standin
tween the undissociated normal molecules and colloid ferric hyd
ide.

Some measurements which were made with ferric nitrate an
lphate gave results similar to those obtained with the chloride, but
ey are very much less complete.

The following table contains the measurements made on concen-
ated acid solutions of ferric sulphate and nitrate, and with a
lution of ferric sulphocyanide in ether; owing to the ease with
hich the latter decomposes the numbers are only approximate, they
ill serve, however, to give some notion of the relative magnitude of
absorption.

For comparison the numbers obtained for the concentrated, acid,
ric chloride solution, and for ferric hydroxide are given.

Table XV.—Acid Solutions of Ferric Salts.

ac.	$Fe(NO_3)_2$	½$Fe_2(SO_4)_3$.	$FeCl_3$.	$Fe(OH)_3$.	$Fe(SCN)_3$.
Fe....	0·4221	0·7498	0·3095		0·00148
Acid..	0·633HNO_3.	1·524(½H_2SO_4).	0·814HCl.		in ether.
λ					
660	0·0492	0·0835	--	—	211
640	0·0534	0·0995	--	—	310
620	0·0600	0·1143	—	—	428
600	0·0675	0·1300	—	—	574
580	0·0771	0·1428	—	39·3	821
560	0·0863	0·1560	0·117	56·6	1152
540	0·0923	0·1644	0·135	85·0	1527
520	0 0920	0·1583	0·290	127·7	1926
500	0·0877	0·1367	0·775	181·7	2503
80	0·0888	0·1255	2·950	221·7	
60	0·1072	0·1390	11·01	308·1	
40		0·7300			

l be noticed that the different salts of iron all have different
on spectra, the nitrate is very slightly coloured indeed; the
brown, and the chloride bright yellow. Ferric hydroxide
several hundred times more light than the salts, and ferric
nide absorbs nearly twenty times as much light as the
.

Appendix.—Table I.

T is the light transmitted by a tube full of water 100·1 cm. long, as directly determined.

Wave-length.	Mean.	T.	Wave-length.	Mean.	T.
752 —708	730	0·2447	589·2—571·5	580	0·8332
752 —708	730	0·2471	582 —576·8	579·4	0·8228
740 —697	718·5	0·3060	572 —562·5	567·2	0·8471
708 —692	700	0·4497	572 —562·5	567·2	0·8424
693 —676	684·5	0·5460	568·5—562·5	565·5	0·8499
693 —673	683	0·5445	570 —561	565·5	0·8363
693 —668	680·5	0·5396	551·5—540	545·7	0·8650
679 —666·5	672·7	0·5983	551·5—540	545·7	0·8625
681·5—658·5	670	0·5904	523 —517·2	520·1	0·8675
678 —661·5	669·7	0·5984	519·7—510·2	514·9	0·8696
663 —651·5	657·2	0·6215	513·3—506·7	510	0·8749
646·5—626	636·2	0·6586	513·3—506·7	510	0·8700
641·5—627	634·2	0·6661	507·5—501	504·2	0·8763
636·5—622	629·2	0·6777	496·2—487·5	491·8	0·8705
636·5—622	629·2	0·6596	488·3—481·5	484·9	0·8752
629 —620·7	624·8	0·6830	468·3—461·5	464·9	0·8694
623 —603	613	0·6915	468·3—461·5	464·9	0·8742
612 —594·2	603·1	0·7057	460·3—455·3	457·8	0·8632
607·5—592·3	599·9	0·7185	454·2—446·8	450·5	0·8689
603·5—589	595·7	0·7260	446·7—441·2	443·9	0·8702
597·5—590	593·7	0·7365	432 —428	430	0·8456
598·5—584	591·2	0·7868	432 —427·8	429·9	0·8296
593·5—578·7	586·1	0·7925	433·5—426·4	429·9	0·8277
593·5—578·7	586·1	0·7855	427·3—420	423·6	0·8178
590 —576·7	583·3	0·8117	418 —411	414·5	0·8147

Table II.

The letters have the following significations:—

c = concentration of the solution in gram-molecules per litre. The molecular weight used is given after the formula of the substance.

l = length of the layer of solution.

T = light transmitted by the tube of solution.

ϵ = extinction coefficient for 1 cm. of solution containing 1 gram-molecule per litre.

I.—$CuSO_4$ = 159·6.
c = 0·003768.
l = 100·1 cm.

Wave-length.	Mean λ.	T.	ε.
657·3—647·5	652·4	0·0576	2·764
637 —625·8	631·4	0·1400	1·806
618 —608	613	0·2521	1·165
603 —592·7	597·8	0·3852	0·7166
587·2—578·5	582·8	0·5292	0·5004
570 —564	567	0·6317	0·3339
558·3—553·7	556	0·7119	0·2129
547·2—542	544·6	0·7568	0·1514
519 —510·2	514·6	0·8404	0·0407
486 —478	482	0·8733	0·00127

II.—$CuSO_4$.
c = 0·00366.
l = 100·1 cm.

Wave-length.	Mean λ.	T.	ε.
512·7—508	510·8	0·8357	0·0477
535 —528	531·5	0·7961	0·1018
558·5—551	554·2	0·7108	0·2230
570 —564	567	0·6131	0·3791
584 —578	581	0·5369	0·5041
601·5—592·5	597	0·3649	0·8066
618 —608	613	0·2331	1·293
638 —625·5	631·7	0·1314	1·934
657·5—645	651·2	0·0495	3·033

III.—$CuSO_4$.
c = 0·003754.
l = 100·25 cm.

Wave length.	Mean λ.	T.	ε.
643·5—632	637·7	0·0931	2·264
621 —613·5	617·2	0·2004	1·426
606·5—597	601·7	0·3106	0·9070
589·5—582	585·7	0·4801	0·5958
575 —565·5	570·2	0·5930	0·4020
561 —555·5	558·5	0·6802	0·2631
538·5—531	534·7	0·7752	0·1286
516 —510·5	513·2	0·8437	0·0372
484 —476·5	480·2	0·8693	0·0061

IV.—$CuCl_2$ = 134·6.
c = 0·003969.
l = 100·1 cm.

Wave-length.	Mean λ.	T.	ε.
486 —478·5	482·2	0·8728	0·00176
519 —510·5	514·7	0·8423	0·0360
537 —529·5	533·2	0·7879	0·1052
547 —542	544·7	0·7402	0·1715
559·5—554·5	557	0·6870	0·2394
570 —564	567·2	0·6085	0·3577
586 —578·5	582·2	0·4931	0·5535
602·5—592·5	597·5	0·3446	0·8029
615·5—608	611·7	0·2131	1·292
637 —627·5	632·2	0·1127	1·950
657 —647·5	652·2	0·0441	2·916

Table II—*continued.*

V.—$Cu(NO_3)_2$ = 187·6.
c = 0·003964.
l = 100·1 cm.

Wave-length.	Mean λ.	T.	ε.
656 —645	650·5	0·0519	2·749
636 —623·5	629·7	0·1245	1·849
617 —608	612·5	0·2289	1·214
602·5—591	596·7	0·3551	0·7600
586 —578·5	582·2	0·5055	0·5265
570 —564·5	567·2	0·6247	0·3284
548 —542	545	0·7718	0·1220
519 —511	515	0·8478	0·0290

VI.—$Cu(NO_3)_2$.
c = 0·003819.
l = 100·25 cm.

Wave-length.	Mean λ.	T.	ε.
662 —651	656·5	0·0292	3·4785
639·5—629	634·2	0·0926	2·2416
620 —612	616	0·1963	1·4264
604 —594·5	599·2	0·3123	0·9364
587 —579·5	583·2	0·4665	0·6332
572 —565	568·5	0·5924	0·3965
560 —554·5	557·5	0·6735	0·2703
536 —529·5	532·7	0·7767	0·1254
514 —509	511·5	0·8197	0·0695
482 —476·5	479·2	0·8618	0·0133

VII.—$Cu(NO_3)_2$ = 187·6.
c = 0·00564.
l = 100·25 cm.

Wave-length.	Mean λ	T.	ε
639 —623·5	631·2	0·0579	1·8827
618·5—608·7	613·6	0·1439	1·2078
603·3—591	597·1	0·2655	0·7671
570 —563	566·5	0·5496	0·3299
545·7—540	542·8	0·7236	0·1360
512 —508	510	0·8419	0·0269

VIII.—$CuBr_2$ = 223·6.
c = 0·005304.
l = 100·1 cm.

Wave-length.	Mean λ.	T.	ε
637 —625	631	0·0533	2·0738
617 —607	612	0·1373	1·3264
601·5—592	596·7	0·2558	0·8498
586·5—578	582·2	0·4132	0·5583
568·5—562·5	565·5	0·5288	0·3837
557 —549	558	0·6346	0·2430
533 —527	530	0·7750	0·0921
511·5—506·5	509	0·8342	0·0362
481 —474·5	477·7	0·8689	0·0041

IX.—$CuBr_2$.
c = 0·003303.
l = 100·25 cm.

Wave-length.	Mean λ.	T.	ε
668 —653	665·5	0·0436	3·4522
643 —633·5	638·2	0·1234	2·2028
622 —612·5	616·2	0·2279	1·4536
605·5—595	600·2	0·3526	0·9175
589·5—581	585·2	0·5064	0·6116
573·5—567·5	570·5	0·6272	0·3814
560·5—555	557·7	0·7051	0·2519
538 —531	534·5	0·7976	0·1087
483·5—477	480·2	0·8690	0·0073

X.—$Cu(C_2H_3O_2)_2$ = 181·6.
c = 0·003748.
l = 100·1 cm.

Wave-length.	Mean λ.	T.	ε
636·5—624·5	630·5	0·0147	4·4274
616 —609	612·5	0·0494	3·0594
602·5—592	597·2	0·1261	2·0170
586 —578	582	0·2569	1·3402
571·5—565	568·2	0·4068	0·8417
559 —554	556·5	0·5226	0·5704
547 —541	544	0·6436	0·3396
537·5—531	534·2	0·7173	0·2188
516 —510	513	0·8217	0·0672

Table II—*continued.*

XI.—$Cu(C_2H_3O_2)_2$.
c = 0·004203.
l = 100·1 cm.

Wave-length.	Mean λ.	T.	ε
480 —474·5	477·2	0·8642	0·0114
510·7—506·4	508·5	0·8243	0·0582
533·5—525·8	529·7	0·7206	0·1921
555·5—547·6	551·6	0·5549	0·4516
568 —560·5	564·2	0·4110	0·7452
582·7—574·5	578·6	0·2633	1·1811
596·5—590	593·2	0·1305	1·797
613·5—605	609·2	0·0466	2·794
634·5—622	628·2	0·0157	3·885

XII.—$Cu(C_2H_3O_2)_2$.
c = 0·0007144.
l = 100·25 cm.

Wave-length.	Mean λ.	T.	ε
658·5—646·5	652·5	0·2981	4·595
639 —625	632	0·4069	3·038
603 —591·5	597·2	0·6183	0·9285
572 —563	567·5	0·7764	0·5055

XIII.—$Cu(C_2H_3O_2)_2$.
c = 0·00289
+ 0·00885 g. mol. acetic acid.
l = 100·25 cm.

Wave-length.	Mean λ.	T.	ε
620·5—611	615·7	0·1003	2·893
588 —580	584	0·3658	1·196
561 —555	558	0·6074	0·5108

XIV.—$Cu(C_2H_3O_2)_2$.
c = 0·00479
+ 0·136 g. mol. acetic acid.
l = 100·25 cm.

Wave-length.	Mean λ.	T.	ε
622 —609	615·5	0·0151	3·460
586·5—577	581·7	0·1437	1·574
534 —527·5	530·7	0·6452	0·2126

XV.—$Cu(C_2H_3O_2)_2$.
c = 0·0007395
+ 0·00155 g. mol. acetic acid.
l = 100·25 cm.

Wave-length.	Mean λ.	T.	ε
658·5—646·5	652·5	0·2874	4·651
639 —625	632	0·3947	3·113
603 —591·5	597·2	0·5968	1·103
572 —563	567·5	0·7801	0·460

Table III.

XVI.—$C_6H_3(NO_2)_2OK$ = 222.
c = 0·001646.
l = 100·25 cm.

Wave-length.	Mean λ.	T.	ε.
680·5—659	669·8	0·5675	0·1248
640·5—620	630·2	0·6083	0·2703
604 —588	596	0·6788	0·1806
572 —560	566	0·7716	0·2400
548 —536	542	0·6397	0·7903
537 —526	531·5	0·4421	1·7726
527 —517	522	0·1798	4·144

XVIII.—$C_6H_3(NO_2)_2OK$.
c = 0·0001027.
l = 25·11 cm.

Wave-length.	Mean λ.	T.	ε.
494 —486·5	490·2	0·5635	77·4
486·5—478·5	482·5	0·3626	151·8
480 —473	476·5	0·1844	265·4
474 —466	470	0·0834	398·6

XXI.—$C_6H_3(NO_2)_2ONH_4$.
c = 0·004739.
l = 25·11 cm.

Wave-length.	Mean λ.	T.	ε,
528 —520·3	524·2	0·4335	2·652
538 —531·5	534·8	0·6663	1·084
549·5—541	545·2	0·7820	0·499
562·7—554·5	558·6	0·8188	0·322

XVII.—$C_6H_3(NO_2)_2OK$.
c = 0·0004107.
l = 100·25 cm.

Wave-length.	Mean λ.	T.	ε.
532·5—521	526·8	0·6921	2·392
522 —512	517	0·5328	5·159
512 —502·5	507·2	0·3105	10·88
502 —494·5	498·2	0·0961	23·26

XIX.—$C_6H_3(NO_2)_2OK$.
c = 0·0001027.
l = 100·25 cm.

Wave-length.	Mean λ.	T.	ε.
510 —502	506	0·5996	15·78
502·3—493·5	497·9	0·4114	31·7
493 —486	489·5	0·1848	65·5
486·5—478	482·2	0·0413	128·7

XX.—$C_6H_3(NO_2)_2OK$.
c = 0·0000986.
l = 100·25 cm.

Wave-length.	Mean λ.	T.	ε.
511·3—505	508·2	0·6449	13·22
503·5—498·8	501·2	0·4937	24·99
487 —482·5	484·8	0·0646	114·3
484·5—479·5	482	0·0345	141·9

XXII.—$C_6H_3(NO_2)_2ONH_4$.
c = 0·001185.
l = 100·25 cm.

Wave-length.	Mean λ.	T.	ε.
518·5—511·5	515	0·1243	7·108
528 —520·5	524·2	0·3640	3·177
539 —530	534·5	0·5615	1·583
552 —541	546·5	0·6477	1·045

Table III—*continued.*

XXIII.—$C_6H_3(NO_2)_2ONH_4$. c = 0·001185. l = 25·11 cm.				XXIV.—$C_6H_3(NO_2)_2ONH_4$. c = 0·0004027. l = 6·08 cm.			
Wave-length.	Mean λ.	T.	ε.	Wave-length.	Mean λ.	T.	ε.
539 —530	534·5	0·7918	1·758	469·5—466	467·8	0·0744	441·1
526·7—521	523·8	0·7441	2·736	476·5—472·7	474·6	0·2022	264·0
518 —511	514·5	0·5580	6·920	484·5—478·5	481·5	0·3844	150·5
508 —502	505	0·2919	16·37	491 —486·5	488·8	0·5631	83·2
499 —493	496	0·0798	35·26				

Table IV.—Dinitrophenol (Acid Solution.)

c = 0·001202 gram molecule dinitrophenol. } per litre.
+ 0·2132 " " HCl.
l = 100·25 cm.

Wave length.	Mean λ.	T.	ε.
517·5—510	513·8	0·7884	0·3577
498·5—492	495·2	0·7783	0·4149
481 —478	479·5	0·7444	0·5776
467·3—464	465·6	0·6956	0·8100
467·5—464	465·8	0·7110	0·7328
457 —451·5	454·2	0·5396	1·713
446 —440	443	0·3680	3·0606
507 —500	503·5	0·7963	0·3303

Table V.—Dinitrophenol.

e length.	Mean λ.	Concentration.	l.	T.	ε.
—478	480·2	0·001581	6·08	0·3194	46·69
—493	496	—	—	0·7316	9·44
—495	497·5	—	25·11	0·4380	7·829
—495	497·7	—	—	0·4977	6·421
—485·3	487·2	—	—	0·1335	20·8
—477	479·2	0·0007905	6·08	0·4539	61·58
—490·7	493·3	—	25·11	0·5285	11·52
—478·5	480·8	—	—	0·1650	36·93
—491·2	493·6	0·000395	25·11	0·6055	17·11
—475	476·8	—	—	0·1337	83·07
470	472·1	0·0001581	25·11	0·1992	163·9
490·5	493·2	—	—	0·7200	23·83
493	496·2	0·00001581	100·25	0·7761	32·3
472	473·8	—	—	0·3153	278·5
477·7	480·1	0·002886	6·08	0·2378	32·86
76·5	479	0·002886	6·08	0·2329	33·37

Table VI.

XXV.—$FeCl_3$ = 162·5.
c = 0·00002525
l = 25·11 cm.

Wave-length.	Mean λ.	T.	ε.
450 —443	446·5	0·5364	342·3
474 —466	470	0·5981	272·4
486·5—478·5	482·5	0·6627	204·9

c = 0·00002525
l = 100·25 cm.

Wave-length.	Mean λ.	T.	ε.
503 —496	499·5	0·3265	168·5
522 —515	518·5	0·4133	127·2
569·5—558	563·8	0·6196	53·6
598 —585	591·5	0·6298	32·1

XXVIII.—$FeCl_3$.
c = 0·000101.
l = 6·08 cm.

Wave-length.	Mean λ.	T.	ε.
460·5—454·5	457·5	0·5866	296·4
480 —473	476·5	0·6857	190·2
503·5—495·5	499·5	0·7356	144·6

c = 0·000101.
l = 25·11 cm.

Wave-length.	Mean λ.	T.	ε.
514·2—503·5	508·9	0·4067	135·2
534 —524	529	0·5122	95·9

XXX*a*.—c = 0·000505.
l = 6·08 cm.

Wave-length.	Mean λ.	T.	ε.
473·5—466·3	469·9	0·1473	255·2
487·5—478·5	483	0·1714	234·2
503·5—495·5	499·5	0·2735	168·7
523 —514·3	518·7	0·3987	115·7
570·5—558	564·2	0·7288	30·6

XXVI.—$FeCl_3$.
c = 0·000332
+ 0·000407 gr. mol. NH_3.
l = 25·11 cm.

Wave-length.	Mean λ.	T.	ε.
502·3—496·3	499·3	0·0192	200·5
507 —502	504·5	0·0314	174·6
517·3—511	514·2	0·0508	149·5
527·5—521	524·2	0·0898	119·9
539·5—530·5	535	0·1443	95·1
550·5—544	547·2	0·2165	73·9
576·5—568·5	572·5	0·4064	40·7

XXVII.—$FeCl_3$.
c = 0·000101.
l = 25·11.

Wave-length.	Mean λ.	T.	ε.
472·5—465	469·2	0·2342	228·5
486 —478	482	0·2803	198·3
502 —493	497·5	0·3574	157·2
522 —511·5	516·8	0·4444	120·1
544 —534	539	0·5713	77·1
584·5—570	577·2	0·7316	33·2

XXIX.—$FeCl_3$.
c = 0·00036.
l = 25·11.

Wave-length.	Mean λ.	T.	ε.
502·3—496·3	499·3	0·0328	158·9
507 —502	504·5	0·0381	151·7
517·3—511	514·2	0·0664	125·0
527·5—521	524·2	0·1063	102·4
539·5—530·5	535	0·1789	77·4
550·5—544	547·2	0·2473	61·8
576·5—568·5	572·5	0·4173	36·3

Table VII.—Colloid Ferric Hydroxide.

The concentration is given in gram molecules per litre, a gram molecule being aken as that quantity which contains 1 atom of iron.

$Fe(OH)_3$.
$c = 0·09855$.
$l = 0·338$ cm.

Wave-length.	Mean λ.	T.	ε
545 —536	540·5	0·0293	44·5
557 —547	552	0·0702	33·1
570 —558·5	564·2	0·1402	23·9
584 —570	577	0·2227	17·87
600 —584	592	0·2980	14·32
616 —600	608	0·3767	10·91
634·5—616	625·2	0·4142	9·63
674·5—656	665·2	0·5409	6·12

$c = 0·000568$.
$l = 25·11$ cm.

Wave-length.	Mean λ.	T.	ε
521—511	516	0·0469	89·98
532—521·5	526·8	0·0966	67·81
555—542	548·5	0·2736	36·07
577—564	570·5	0·4590	20·94
606—592	599	0·5666	12·52

Ferric Hydroxide.
$c = 0·0001254$.
$l = 25·11$ cm.

Wave-length.	Mean λ.	T.	ε
·5—562·5	570	0·7583	22·23
·5—508	513·2	0·4118	107·2
—475·5	480·2	0·2056	202·4

$c = 0·0001254$.
$l = 6·08$ cm.

Wave-length.	Mean λ.	T.	ε
—421·5	423·8	0·5590	263·5
—439·5	442·8	0·4132	431·6

$Fe(OH)_3$.
$c = 0·02531$.
$l = 0·932$ cm.

Wave-length.	Mean λ.	T.	ε
533 —523	528	0·0324	61·05
544·5—534	539·2	0·0782	47·76
555·5—545	550·2	0·1471	33·04
567 —557	562	0·2357	24·29
594 —582	588	0·4009	14·35
628·5—613·5	621	0·5279	9·16
674·5—653	663·8	0·6218	5·61

$c = 0·000568$.
$l = 6·08$.

Wave-length.	Mean λ.	T.	ε
436 —432·3	434·1	0·0691	320·7
446 —442·5	444·2	0·1089	263·8
464 —458·5	461·2	0·1598	216·3
486 —478·5	482·2	0·1920	193·9
500·5—494	497·2	0·2804	146·8
519 —511	515	0·4260	94·4
542 —530	536	0·5926	53·1

Table VIII.—Acid Solutions of Ferric Chloride.

XXX.—$FeCl_3$ = 0·001995 gram. mol.
HCl = 0·00466.
l = 100·25 cm.

Wave-length.	Mean λ	T.	ε.
597·5—585	591·2	0·7322	0·0890
558 —547	552·5	0·7955	0·1700
523·5—516	519·8	0·7172	0·4185
490 —482	486	0·5770	0·9000
475·5—468	471·8	0·4815	1·289
462 —455	458·5	0·3900	1·744
450 —445	447·5	0·2907	2·368
452 —445·5	449	0·2929	2·353
430·5—428·8	428·8	0·1056	4·500

XXXIII.—$FeCl_3$ = 0·00425.
HCl = 0·06.
l = 100·25 cm.

Wave-length.	Mean λ	T.	ε.
439 —434	436·5	0·0456	2·984
444 —438	441	0·1231	1·979
462 —454·2	458·1	0·4558	0·658
487·5—480·5	484	0·7550	0·149
520·5—516	518·2	0·7995	0·086
566·5—560·5	563·5	0·7769	0·090

XXXIV.—$FeCl_3$ = 0·0013.
HCl = 0·035.
l = 100·25 cm.

Wave length.	Mean λ.	T.	ε.
431 —426·3	428·6	0·2640	3·851
446·8—439·8	443·3	0·5879	1·272
462·7—456·2	459·5	0·7472	0·507
482·6—478·5	480·5	0·8197	0·214
517 —511	514	0·8388	0·124
561·6—554	557·8	0·8392	0·062

XXXI.—$FeCl_3$ = 0·001284.
HCl = 0·0094.
l = 100·25.

Wave-length.	Mean λ	T.	ε.
429·6—424·5	427·1	0·3150	3·254
439 —434·3	436·7	0·4713	1·999
453·5—448·5	451·0	0·6548	0·949
488·5—480	484·2	0·7802	0·382
544·3—536	540·2	0·8077	0·232

XXXII.—$FeCl_3$ = 0·001279.
HCl = 0·0178 cm.
l = 100·25.

Wave-length.	Mean λ.	T.	ε.
422·5—415·5	419	0·1903	4·950
435 —424·5	429·8	0·3391	3·078
446 —438·5	442·2	0·5861	1·299
464 —457	460·5	0·6939	0·767
482·5—477·5	480	0·8101	0·299
507·5—501·5	504·5	0·8304	0·169
534 —528	531	0·8389	0·116
568·5—562·5	565·5	0·8144	0·129

XXXV.—$FeCl_3$ = 0·3095.
HCl = 0·814.
l = 6·08 cm.

Wave length.	Mean λ.	T.	ε.
511·5—506	508·8	0·1200	0·4658
520·3—517	518·6	0·2718	0·2772
530·5—525·5	528	0·4030	0·1864
567·5—561	564·2	0·5380	0·1197
l = 0·932 cm.			
479·5—475·5	477·5	0·0808	3·6025
486·5—482	484·2	0·2040	2·2074
494 —490	492	0·3524	1·3846
502·5—498·5	500·5	0·5379	0·7480
512 —508	510	0·6445	0·4758
522 —517·5	519·8	0·7108	0·3284
l = 0·338 cm.			
480 —473	476·5	0·3878	3·4210
470 —463·5	466·8	0·1643	6·9865
460·5—454·5	457·5	0·0316	13·825

Table IX.

ic Sulphate (Acid Solution).
498 gram molecule ferric sulphate ($\frac{1}{6}$Fe$_2$(SO$_4$)$_3$) + 1·528 gram eq. $\frac{1}{2}$H$_2$SO$_4$).
·4 cm.

ength.	Mean λ.	T.	ε.
661	571·5	0·4623	0·0756
610·5	618·5	0·4060	0·0895
572	579·2	0·2494	0·1433
524	529·5	0·1953	0·1650
495·5	499·8	0·2579	0·1369

l = 0·257 cm.

467	470·5	0·9449	0·1281
455	457·8	0·9383	0·1436
438	440·5	0·7277	0·7180

Ferric Nitrate (Acid Solution).
0·422 gram molecule Fe(NO$_3$)$_3$.
0·633 „ „ HNO$_3$.
l = 24·77 cm.

Wave length.	Mean λ.	T.	ε.
680 —660	670	0·2876	0·0474
587 —570	578·5	0·1503	0·0780
524 —515	519·5	0·1083	0·0919

l = 5·74 cm.

486·5—480	483·2	0·6131	0·0874
459·5—454	456·8	0·5297	0·1136

Ferric Sulphocyanide (Ether Solution).
c = 0·00148 gram molecule Fe(SCN)$_3$.
l = 0·34 cm.

Wave length.	Mean λ.	T.	ε.
603·2—588	595·6	0·0352	2769·3
618·5—603·2	610·8	0·0973	1871·7
638·5—622	630·2	0·2305	1147·3
650 —630	640	0·3348	825·1
669 —651	660	0·5308	427·5
695 —669	682	0·6983	190·8

December 6, 1894.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced that he had appointed as Vice-Presidents—

The Treasurer.
Sir John Kirk.
Professor J. B. Sanderson.
Professor T. E. Thorpe.

The following Papers were read:—

I. "Experimental Researches on Vegetable Assimilation and Respiration.—No. I. On a New Method for Investigating the Carbonic Acid Exchanges of Plants." By F. F. Blackman, B.Sc., B.A., St. John's College, Demonstrator of Botany in the University of Cambridge. Communicated by Francis Darwin, F.R.S. Received November 15, 1894.

(Abstract.)

All the processes hitherto available for the estimation of carbon dioxide in its biological relations are open to serious objections, either on the score of the amount of time involved in their performance, or of their inadaptability to the estimation of small quantities of carbon dioxide when slowly evolved.

The present communication describes an apparatus in which, as a result of two years' work, I have succeeded in combining a high degree of chemical accuracy with special adaptability to biological research.

Thus by its aid the evolution of CO_2, by a single germinating seed or by a small area of a foliage leaf, can be accurately estimated from hour to hour without a break, for any desired time, while for the same area of leaf, the more active absorption of CO_2 in assimilation can be easily determined for such short periods of time as fifteen minutes, and that at the same time separately for the two surfaces of one and the same leaf area. Further, for the purposes of this assimilation, a current of air containing any desired proportion of CO_2, however small, can be supplied continuously to the tissue under investigation,

while, if desired, estimations of the CO_2 evolved in respiration by some other part can be carried on simultaneously in a separate current of air freed from CO_2. This is made possible by the apparatus being practically in duplicate; strictly comparative experiments can thus be carried out.

The actual estimation of the CO_2 is accomplished by the well-known method of absorption by baryta solution and titration with hydrochloric acid. The novelty consists in this, that only a very small quantity of baryta solution (under 15 c.c.) is employed in each experiment, and that after the absorption the whole of this is titrated with acid in the tube in which the absorption has taken place. Further, the burettes containing the standard solutions are always in air-tight connection with this absorption chamber, and no air beyond the current under investigation is ever admitted to the chamber, except such as has been carefully freed from CO_2. The special arrangements for stirring and emptying, by means of this air, freed from CO_2, and stored under pressure, cannot be entered into here.

The two currents of air passing continuously through the apparatus are generated by two aspirators of a special type, which, worked on the principle of Mariotte's bottle, give a practically constant outflow in drops, whatever the level of the water within them, and are adapted to work steadily with small rates of flow (50 to 100 c.c. per hour). These currents enter the apparatus, either through an arrangement for removing the CO_2 when working on respiration, or when working on assimilation through one for adding CO_2. Both these are so constructed that the current never has to bubble through a layer of liquid, and so is supplied to the plant at strictly atmospheric pressure, thus avoiding any risk of drawing gases mechanically out of the part under investigation. The remover of CO_2 is a " tower " full of beads, over which a stream of strong potash flows continuously, and through which the air is drawn. The CO_2 generator is constructed on a new principle, and consists of a tall tube containing fragments of marble, through which the air current passes at a constant rate, while very dilute HCl trickles down it at an extremely slow rate, which is made constant and independent of external variations of temperature by special arrangements. Thus a constant amount of CO_2 is being continually generated, and is carried off by the air current. The amount of CO_2 formed can be controlled by the strength of the acid employed. When generating amounts below 2 per cent. of the air current, this arrangement works very constantly. From the CO_2 generator or remover, as the case may be, the current of air passes to the receivers, in which the parts of the plant under investigation are situated. These receivers are of various forms, according to the material experimented on, but are all constructed on the cardinal principle of making them as small as possible consistent with the well-being of the part, in order that changes in the composition of the gas shall, as soon as possible, be felt by the current which passes thence through narrow tubes to the absorption chambers. When titrations are being made, and the air current can no longer be allowed to pass through the absorption chambers, it passes through a column of water equal in its resistance to that of the baryta solution in the absorption chamber. This enables the rate of flow to be kept constant between, as well as during, the actual experiments. Numerous other details, such as the special method of refilling the burettes, &c., and above all those small points by which constancy is, as far as possible, attained, many of which have involved weeks of special experiment, cannot be described here.

Simplification of technique by complication of apparatus has been the guiding principle, and the result is that, although the whole consists of at least eight separate pieces of apparatus, many being further in duplicate, and all connected together by a plexus of tubes, yet the working is so automatically arranged that the operator, beyond reading the burettes and occasionally working a finger bellows, has nothing to do but turn stopcocks.

If only one series of estimations is being made, these can be kept absolutely consecutive, the current being led through one of the absorption chambers, while the solution in the other one is being titrated and renewed, and so on alternately. When two series of comparative estimations are being made at once, a small interval must be allowed after each double estimation, during which the titrations are performed ; the currents of air in connection with the plants then pass through bye-paths, still at their previous rate. This interval (in which a double titration, emptying and refilling of the absorption chambers, is accomplished) can be reduced to ten minutes.

Delicacy of estimation sufficient for present work is obtained by the use of half-decinormal, N/20, standard solutions. Phenolphthalein is used as indicator, and specially delicate end-reactions can be obtained, since atmospheric CO_2 is excluded, and moreover the burettes containing *both* the solutions can be drawn upon.

The burettes, narrow and graduated in 1/10 c.c., are read to 1/100 c.c., with a simple arrangement for avoiding parallax. All other usual precautions are taken, and series of control titrations, with an error of observation of not more than 0·1 per cent., are often obtained. This corresponds to 1/200 c.c. CO_2.

In experiments of short duration, 1/50 c.c. CO_2 is found to be sufficient for a trustworthy estimation from which definite conclusions may be drawn.

The communication immediately following the present one, illustrates the applicability of this apparatus to the investigation of minute quantities of carbon dioxide.

II. "Experimental Researches on Vegetable Assimilation and Respiration.—No. II. On the Paths of Gaseous Exchange between Aerial Leaves and the Atmosphere." By F. F. BLACKMAN, B.Sc., B.A., St. John's College, Demonstrator of Botany in the University of Cambridge. Communicated by FRANCIS DARWIN, F.R.S. Received November 15, 1894.

(Abstract.)

On the question of the path by which carbonic acid passes out of the leaf in respiration and into it in assimilation, whether this takes place by the stomatal openings or through the continuous surface of the cuticle, all possible extreme and intermediate views have been expressed in recent text-books of botany. On account of the smallness of the quantities of gas involved, practically no attempt has hitherto been made to determine this question by direct estimation. The existing experimental evidence is all of an indirect nature, and tends rather to support the view that the exchange is a cuticular phenomenon.

An ingenious synthesis of Graham's observations on the comparative readiness with which CO_2 osmoses through thin films of caoutchouc, with observations by Frémy and others on the similarity between cuticle and caoutchouc in chemical composition, first led Barthélemy (1868) to put forward the view that the cuticle was specially adapted for transmitting CO_2 from the external air to the assimilating cells beneath it. This view he supported by experiments on the artificial osmosis of gases through leaves. About the same time Boussingault performed experiments that seemed to definitely show that in assimilation, the CO_2 taken up by the leaf entered it through the upper surface, devoid of stomata, to which the assimilating cells are adjacent, rather than through the more distant stomatal openings. These experiments have hitherto been generally accepted, but I shall show later that the conclusions drawn from them are entirely fallacious. In support of the view that stomata form the paths of gaseous exchange, besides scattered inductions by various workers, we have the conclusion arrived at in 1888 by Mangin, from diffusion experiments on isolated cuticle, that this diffusion is insufficient to account for the whole gaseous exchange of the leaf.

By the aid of the apparatus described in a previous paper, the author has been able successfully to attack the problem directly by estimating the amounts of CO_2 given out or taken in by the two surfaces of the same leaf, under the same conditions. For this pur-

pose, shallow capsules 10 sq. cm. in area, consisting of a glass plate with a metal rim, through which tubes for the circulation of the air current pass, are employed. Two of them are affixed to a leaf on opposite sides of the same area in air-tight union by means of soft wax. Then, in the way described previously, two continuous currents of air can be kept up over the two surfaces, and the CO_2 produced or taken in, during a given time, by each of them be determined.

Numerous experiments on the respiration of a variety of leaves, thick and thin, with the stomata all on the one side or with stomata variously distributed on the two sides, agree in showing that the stomata are the site of the exhalation of this gas. When no stomata are present on the upper surface of a leaf, then practically no CO_2 is exhaled from that surface, at least, not more than falls within the working error of the apparatus under these conditions, while more than thirty times as much may be given off from the lower stomatiferous surface. When stomata occur on both surfaces, the relative amounts of CO_2 exhaled closely follow the ratios of the numbers of stomata.

A few examples in illustration of this may be given.

	A.	B.	C.
Ampelopsis hederacea	$\frac{0}{8}$	$\frac{0{\cdot}003}{0{\cdot}100}$ c.c. CO_2 per hour per 10 sq. cm.....	$\frac{3}{100}$
Alisma Plantago....	$\frac{135}{100}$	$\frac{0{\cdot}030}{0{\cdot}025}$,, ,, ,, 10 sq. cm.....	$\frac{120}{100}$
Iris germanica......	$\frac{100}{100}$	$\frac{0{\cdot}029}{0{\cdot}026}$,, ,, ,, 10 sq. cm.....	$\frac{110}{100}$
Ricinus communis...	$\frac{100}{250}$	$\frac{0{\cdot}015}{0{\cdot}037}$,, ,, ,, 10 sq. cm.....	$\frac{100}{260}$

Column A gives the proportionate numbers of stomata on the two surfaces; B the amounts of CO_2 exhaled by these; C the ratios of these amounts. Each experiment lasted about fifteen hours.

Experiments on the absorption of CO_2 during assimilation showed the same close relation to the distribution of stomata. For these experiments a constant fixed amount of CO_2 must be introduced into the air stream supplied to the leaf, which makes them much more complicated.

As hitherto carried out, direct sunshine, continuous for several hours, has been essential for the success of these experiments. They are, consequently, but few in number, though perfectly clear in their interpretation.

In an experiment on the leaf of *Ampelopsis hederacea*, with no stomata on the upper surface, air containing 1·6 per cent. CO_2 was supplied to both surfaces at the rate of 22 c.c. for every fifteen minutes; of the 0·37 c.c. of CO_2 thus entering the capsule on the lower surface in this time, 0·14 c.c. was absorbed, while none at all was absorbed by the upper surface. With a leaf of *Alisma*, on the contrary, the whole of the CO_2—0·15 c.c.—supplied in fifteen minutes to the upper surface was absorbed, and 0·11 c.c. of that supplied to the lower. In this leaf the stomata are in the ratio of 135 above to 100 below, to which ratio the absorption numbers closely correspond. A very simple experiment will show that stomata are practically the sole path of entry of CO_2 for assimilation. If part of the lower stomatic surface of any leaf with no stomata on its upper surface (*Sparrmannia* gives very clear results) be coated with wax so as to mechanically block the stomata, no starch can be formed in that area, while the adjacent areas become rich in starch. I performed this experiment in 1893, and showed it to some botanists; recently it has been published by Stahl.*

As stated previously, the theory of "cuticular exchange" has hitherto found its strongest support in the experiments of Boussingault, in which, under similar conditions, leaves of *Nerium Oleander* assimilated less when the upper astomatiferous surface had been coated with an unguent than when the lower stomatiferous surface had been so coated. From this he drew the obvious conclusion that the CO_2 of assimilation normally passes into the leaf through the cuticle of the upper surface. Exposure of the interesting experimental fallacy here concealed, however, quite reverses the interpretation of these experiments. Boussingault experimented with leaves in an atmosphere containing 30 per cent. CO_2. Now the optimum percentage of CO_2 for assimilation is very low for this leaf, and the real interpretation of the result is that the diminished decomposition of CO_2 in the leaf with open stomata is due to its obtaining not *less* CO_2 but *more* CO_2. In fact, there penetrates into it so much CO_2 that its assimilatory activity is lessened, and falls below that of the other leaf into which, owing to the blocking of the stomata, the CO_2 only diffuses very slowly, and cannot exceed the optimal strength. This view has been conclusively proved by a series of experiments in different strengths of CO_2. In a small percentage the leaf with its stomata open decomposes more CO_2 than the leaf with its stomata blocked—a result just the reverse of Boussingault's.

Further evidence on the possible paths of gaseous exchange has been obtained by investigating the degree to which diffusion of CO_2 can be artificially produced through the living leaf. Strong mixtures of CO_2 are led continuously across one surface of a leaf, and the amount which diffuses through is estimated. On supplying 31 per cent. CO_2 to the lower surface of a leaf of *Nerium* only 0·035 per cent. appears by diffusion in a slow current of air kept continually passing over the upper surface of the leaf.

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* 'Botanische Zeitung,' July, 1894.

Other experiments on the respiration of injected leaves also support the view that the stomatal openings, in spite of their minuteness, offer a very much easier path from the atmosphere to the interior of the leaf than does the cuticle.

Conclusions.—1. Under normal conditions practically the sole pathway for CO_2 into or out of the leaf is by the stomata. Since oxygen diffuses more readily than CO_2 through fine openings, the same probably holds for oxygen and the whole of the gas exchange.

2. Under abnormal conditions, when the stomata or intercellular spaces are blocked and the surrounding tension of CO_2 is great enough, passage of CO_2 by osmosis through the cuticle may take place.

3. That such closure of stomata as is held to take place in darkness does not prevent the distribution of gas exchange closely agreeing with that of the stomata.

4. That the exhalation of CO_2 in bright light by a leafy shoot in Garreau's well-known experiment is not the expression of any physiological truth for the leaf, but only due to the imperfections of the conditions; to the presence of immature parts, or of tissues not sufficiently green or not fully illuminated. Mature isolated green leaves fully illuminated assimilate the whole of their respiratory CO_2 and allow none to escape from them.

Presents, December 6, 1894.

Transactions.

Boston:—American Academy of Arts and Sciences. Proceedings. Vol. XXIX. 8vo. *Boston* 1894. The Academy.

Brisbane:—Queensland Branch, Royal Geographical Society of Australasia. Proceedings and Transactions. Vol. IX. 8vo. *Brisbane* 1894. The Branch.

Cambridge, Mass.:—Museum of Comparative Zoology. Bulletin. Vol. XXV. Nos. 9—10. 8vo. *Cambridge* 1894. The Museum.

Cracow:—Académie des Sciences. Bulletin International. Octobre, 1894. 8vo. *Cracovie.* The Academy.

Dublin:—Royal Irish Academy. Transactions. Vol. XXX. Parts 13—14. 4to. *Dublin* 1894. The Academy.

Edinburgh:—Botanical Society. Transactions and Proceedings. Vol. XX. Part 1. 8vo. *Edinburgh* 1894. The Society.

Transactions (*continued*).

Edinburgh Geological Society. Transactions. Vol. VII. Part 1. 8vo. *Edinburgh* 1894. The Society.

Glasgow:—Philosophical Society. Proceedings. Vol. XXV. 8vo. *Glasgow* 1894. The Society.

Halifax, N.S.:—Nova Scotian Institute of Science. Proceedings and Transactions. Second Series. Vol. I. Part 3. 8vo. *Halifax* 1893. The Institute.

Halle:—Verein für Erdkunde. Mitteilungen. 1894. 8vo. *Halle.* The Society.

Jena:—Medizinisch-Naturwissenschaftliche Gesellschaft. Jenaische Zeitschrift. Bd. XXIX. Heft 1. 8vo. *Jena* 1894. The Society.

Kingston:—Institute of Jamaica. Journal. Vol. II. No. 1. 8vo. *Kingston* 1894. The Institute.

London:—Anthropological Institute. Journal. Vol. XXIV. No. 2. 8vo. *London* 1894. The Institute.

Aristotelian Society for the Systematic Study of Philosophy. Proceedings. Vol. II. No. 3. Part 2. 8vo. *London* 1894. The Society.

British Association for the Advancement of Science. List of Officers, Council, and Members. 1894. 8vo. *London.* The Association.

British Astronomical Association. Journal. Vol. IV. No. 11. 8vo. *London* 1894. The Association.

Clinical Society. Transactions. Vol. XXVII. 8vo. *London* 1894. The Society.

Royal Meteorological Society. Quarterly Journal. Vol. XX. No. 92. 8vo. *London* 1894; The Meteorological Record. Vol. XIV. No. 53. 8vo. *London* 1894. The Society.

Royal United Service Institution. Journal. Vol. XXXVIII. No. 201. 8vo. *London* 1894. The Institution.

Madras:—Government Museum. Bulletin. No. 2. 8vo. *Madras* 1894. The Museum.

Moscow:—Société Impériale des Naturalistes. Bulletin. Année 1894. No. 2. 8vo. *Moscou.* The Society.

Naples:—Accademia delle Scienze Fisiche e Matematiche. Rendiconto. Anno XXXIII. Fasc. 8—10. 4to. *Napoli* 1894. The Academy.

New York:—American Museum of Natural History. Bulletin. Vol. VI. Pages 305—320. 8vo. [*New York*] 1894. The Museum.

Philadelphia:—American Philosophical Society. Proceedings. Vol. XXXIII. No. 145. 8vo. *Philadelphia* 1894. The Society

Transactions (*continued*).

St. Louis:—Academy of Sciences. Transactions. Vol. VI. Nos. 9—17. 8vo. [*St. Louis*] 1893–94. The Academy.

Salem:—Essex Institute. Bulletin. Vol. XXV. Nos. 4—12. Vol. XXVI. Nos. 1—3. 8vo. *Salem* 1893–94. The Institute.

Santiago:—Société Scientifique de Chili. Actes. Tome III. Livr. 4—5. Tome IV. Livr. 2. 8vo. *Santiago* 1894. The Society.

Tōkyō:—Imperial University of Japan. Calendar. 1893–94. 8vo. *Tōkyō* 1894; Journal of the College of Science. Vol. VIII. Part 1. 4to. *Tōkyō* 1894. The University.

Wellington, N.Z.:—Polynesian Society. Journal. Vol. III. No. 3. 8vo. *Wellington, N.Z.* 1894. The Society.

Observations and Reports.

Austria:—Österreichische Gradmessungs-Commission. Verhandlungen. April, 1894. 8vo. *Wien.* The Commission.

Baltimore:—Maryland State Weather Service. Monthly Report. Vol. III. No. 12. Vol. IV. Nos. 1—3. 8vo. *Baltimore* 1894. Weather Bureau, Washington.

Bombay:—Colába Observatory. Report on the Condition and Proceedings for the year ended June 30th, 1894. Folio. *Bombay.* The Observatory.

Cape of Good Hope:—Royal Observatory. Report of Her Majesty's Astronomer. 1889–93. 4to. *London* 1893–94. H.M. Astronomer, Cape Town.

Chicago:—World's Columbian Exposition. The United States of Venezuela in 1893. 8vo. *New York.* Smithsonian Institution, Washington.

Coimbra:—Observatorio. Observações Meteorologicas e Magneticas. 1893. Folio. *Coimbra* 1894. The Observatory.

Edinburgh:—Royal Observatory. Circular. No. 43. 4to. *Edinburgh* 1894. The Observatory.

India:—Tide Tables for the Indian Ports for the Year 1895. 2 parts. 8vo. *London.* India Office.

New York:—State Land Survey. Report on the Progress of the Survey. 1891. 8vo. *Albany.* The Superintendent.

Norway:—Norwegische Commission der Europäischen Gradmessung. Resultate der im Sommer 1893 in dem nördlichsten Theile Norwegens ausgeführten Pendelbeobachtungen. 8vo. *Kristiania* 1894. The Commission.

San Fernando:—Instituto y Observatorio de Marina. Almanaque Náutico para 1896. 8vo. *Madrid* 1894. The Observatory.

Observations and Reports (*continued*).

Washington:—Bureau of Equipment. American Ephemeris and Nautical Almanac. 1897. 8vo. *Washington* 1894.
The Bureau.

Surgeon-General's Office, U.S. Army. Index-Catalogue of the Library. Vol. XV. 4to. *Washington* 1894.
The Surgeon-General.

U.S. Department of Agriculture. Monthly Weather Review. August, 1894. 4to. *Washington.* The Department.

U.S. Geological Survey. Annual Report. 1890–92. 4to. *Washington* 1891–93. The Survey.

U.S. Naval Observatory. Observations. 1889. 4to. *Washington* 1893; Meteorological Observations and Results. 1889. 4to. *Washington* 1893; Magnetic Observations. 1892. 4to. *Washington* 1893. The Observatory.

Wellington, N.Z.:—Ministry of Mines. Papers and Reports relating to Minerals and Mining. Folio. *Wellington* 1894.
The Minister.

Journals.

Agricultural Gazette of New South Wales. Vol. V. Part 9. 8vo. *Sydney* 1894. Department of Agriculture, Sydney.

American Journal of Philology. Vol. XV. No. 3. 8vo. *Baltimore* 1894. The Editor.

Boletin de Minas Industria y Construcciones. Año X. Num. 8. 4to. *Lima* 1894. Escuela Especial de Ingenieros, Lima.

Horological Journal. Vol. XXXVII. No. 436. 8vo. *London* 1894. British Horological Institute.

Medico-Legal Journal. Vol. XII. No. 1. 8vo. *New York* 1894.
The Editor.

Revue Médico-Pharmaceutique. Année VII. No. 10. 4to. *Constantinople* 1894. The Editor.

Stazioni Sperimentali Agrarie Italiane. Vol. XXVII. Fasc. 2. 8vo. *Modena* 1894. R. Stazione Agraria, Modena.

Year-Book of Pharmacy. 1894. 8vo. *London.*
British Pharmaceutical Conference.

Bramwell (B) Atlas of Clinical Medicine. Vol. III. Part 1. Folio. *Edinburgh* 1894. The Author.

Haeckel (E.) Systematische Phylogenie der Protisten und Pflanzen. Theil 1. 8vo. *Berlin* 1894. The Author.

Hinrichs (G. D.) The Elements of Atom-Mechanics. Vol. I. 8vo. *St. Louis, Mo.* 1894. The Author.

Howell (J. W.) The Unity of Nature; compiled from his posthumous papers by C. Pooley. 8vo. *London* 1849. Mr. H. B. Woodward.

Hull (E.), F.R.S. Aperçu de la Structure Géologique de l'Arabie Pétrée et de la Palestine. 8vo. *Liége* 1894. The Author.

Hutton (A. W.) The Vaccination Question. 8vo. *London* 1894. The Author.

Kölliker (A. von), For. Mem. R.S. Ueber die feinere Anatomie und die physiologische Bedeutung des Sympathischen Nervensystems. 8vo. *Wien* 1894. The Author.

Reade (T. M.) The Dublin and Wicklow Shelly-drift. 8vo. *Liverpool* 1894. The Author.

Roscoe (Sir H. E.), F.R.S., & Classen, A. Roscoe-Schorlemmer's Kurzes Lehrbuch der Chemie. Zehnte Auflage. 8vo. *Braunschweig* 1894. Sir Henry Roscoe, F.R.S.

Todd (Sir C.), F.R.S. Meteorological Work in Australia: a Review. 8vo. [*Adelaide* 1894.] The Author.

Topley (W.), F.R.S., Memoir of [by H. B. Woodward]. 8vo. *Hertford* 1894. Mrs. W. Topley.

MS. vol. inscribed "Philosophia Universalis pertinens ad Gabrielem Collenno." Sm. 4°. 784 pp. Date, 1694–96. Mrs. W. Topley.

December 13, 1894.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Photographic Spectrum of γ-Cassiopeiæ." By J. NORMAN LOCKYER, C.B., F.R.S. Received November 19, 1894.

Introductory.

In the Bakerian Lecture for 1888* I referred in some detail to the spectrum of γ-Cassiopeiæ, and suggested explanations of the origins of the various lines which had been observed. At that time no photographs of the spectrum were available for discussion, but the questions raised by the eye observations were of such great interest that I determined to attempt a photographic study of the spectrum. This was commenced in November, 1888, and has been continued at intervals up to the present time.

In view of the somewhat conflicting evidence of the variability of the spectrum, it was considered necessary that the photographs should extend over a considerable period before any serious attempt was made to discuss them. Some of the results obtained, however, now appear to be so definite that I have thought it desirable to communicate them to the Society at once. I therefore give particulars of the inquiry so far as it has yet gone, and a brief account of other observations is added, in order that the significance of the photographic results may be the better realised.

In the paper referred to I gave an historical statement including the early observations; it is only necessary to add to this that the spectrum has been since observed or photographed by Professor E. C. Pickering, Professor Keeler, and Dr. Belopolsky.

Professor Pickering makes no mention of dark lines, or of bright lines other than F.†

Professor Keeler, observing with the Lick refractor in 1889‡ failed to see D_3, and gives the following particulars of his observations:—

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* 'Roy. Soc. Proc.,' vol. 44, p. 43.

† 'Draper Catalogue,' p. 266.

‡ 'Publ. Ast. Soc. Pacific,' vol. 1, p. 80.

"I have examined it [the spectrum of the star] frequently, but, so far, no changes have been seen. The C and F lines are brilliant, narrow, and sharp. Hγ in the violet is seen with some difficulty. The green is full of very fine delicate dark lines, seen only under good atmospheric conditions, the *b* group being somewhat more prominent than the others. There is also an appearance of faint bright lines, or perhaps spaces between the fine dark lines just mentioned, seeming bright by contrast. It is difficult to decide on this point. There are in all eight or ten such places. Somewhat nearer to C than to the estimated position of D is a fairly prominent dark band, or, more probably, group of fine lines. Not the slightest trace of bright or dark lines can be seen in the vicinity of D. The continuous spectrum close to the bright hydrogen lines appears somewhat darker than it does elsewhere, but this I have considered to be the effect of contrast."

Professor Keeler has since succeeded in photographing dark lines in the spectrum.*

Belopolsky states that D_3 did not appear in three photographs of the spectrum taken by him in 1892.†

It may also be stated that the spectrum of this star has been occasionally shown to students at Kensington since 1886, and the bright lines C and F have always been visible. Four dates are recorded in the note-books, namely, September 18, October 13 and October 24, 1889, and October 21, 1894. The presence of bright D_3, as well as of C and F, was noted on all these dates, except October 24, 1889.

Dr. Copeland has been good enough to communicate to me an unpublished observation made at Dunecht on September 13, 1885. C was then very bright, F just measurable, while D_3 could not be made out with certainty. This observation was made with the 15-in. refractor.

It appears, therefore, that D_3, and possibly also the bright lines of hydrogen, may be of variable intensity, but it does not seem possible to lay down any law connecting the changes.

The Kensington Photographs.

The first photograph obtained at Kensington was taken on November 20, 1888, and since then fifty-three photographs have been secured. The instruments employed were those described in my recent paper on "The Photographic Spectra of some of the Brighter

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* 'Astronomical Spectroscopy,' Frost's translation, p. 249.

† 'Ast. and Ast. Phys.,' 1893, p. 259.

Stars."* A complete list of the photographs is given in the following table.

Date.	Instrument employed.	Date.	Instrument employed.
1883. Nov. 20	A	1892. May 10	B
" 21	"	" 18	"
1891. Jan. 14	C	June 27	"
" 22	"	" 29	"
Mar. 3	"	July 4	"
July 17	"	July 22	"
Sept. 9	"	Sept. 5	"
" 14	A	" 14	"
" 15	C	Oct. 18	"
Oct. 2	A	1893. Aug. 8	"
" 3	"	" 17	"
" 7	"	" 22	"
" 9	"	" 25	"
" 12	"	" 28	"
" 15	"	Sept. 4	"
" 17	"	" 7	"
" 20	"	Oct. 6	"
" 23	"	" 26	"
" 28	"	Dec. 14	"
" 30	"	" 21	"
Nov. 9	"	1894. Jan. 12	"
" 11	"	" 18	"
" 30	"	Oct. 16	"
Dec. 2	"	Nov. 6	"
1892. Jan. 1	"	" 15	"
" 3	"	" 16	"
" 8	"		

Results of the Preliminary Discussion.

(1) *All the photographs taken at Kensington show bright lines of hydrogen.*

The lines Hβ, Hγ, and Hδ are constantly seen, and Hε and Hζ appear when the photographic conditions have been good. This suggests that their apparent absence, noted by some observers in the period 1874 to 1883, was possibly due to imperfect conditions of observation.

(2) *In addition to the bright lines of hydrogen, there are other bright lines in the spectrum.*

The additional bright lines appear in all good negatives; in cases of under or over exposure, or when other conditions have affected the quality of the negative, they are not distinctly seen.

For the most part, these lines are ill-defined; their positions, therefore, cannot be determined with any great degree of accuracy.

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* 'Phil. Trans.,' A, 1893, vol. 184, p. 678.

The wave-lengths of some of these lines may also require correction for displacement due to motion in the line of sight, when the conditions have been more fully investigated. Attention may be specially directed to two lines at 4384 and 4465; in the spectrum of β-Lyræ there are two bright lines with the normal positions 4388 and 4471, agreeing in position with prominent lines in the spectra of stars of Group IIIγ,[*] and it seems possible that we have to deal with the same lines in γ-Cassiopeiæ. This is rendered more probable by the fact, which will appear later, that the two lines in question appear as bright fringes on the more refrangible sides of dark lines, having the wave-lengths 4388 and 4471.

(3) *During the period covered by the photographs there is no evidence of any change in the intensities of the principal bright lines.*

(4) *The bright lines of hydrogen are double in all the photographs taken with sufficient dispersion.*

The series of twenty-six photographs, showing the duplicity of the hydrogen lines, was taken with the 6-in. prism of 45°, and extends from May 18, 1892, to November 16, 1894.

Hγ and Hδ usually show the doubling best, but in some of the photographs Hζ is also clearly seen to be double. The doubling is not so clearly seen at Hϵ, probably owing to the presence of other lines near the same wave-length. The double lines are somewhat diffused at both edges.

(5) *There is no evidence of orbital movement during the period which has elapsed since May,* 1892.

Careful measurements of the distance between the two bright lines at Hγ and Hδ have been made by Mr. D. Baxandall, and it was found that the distances are constant, within the limits of accuracy in measurement.

(6) *Assuming the presence of two sources of bright hydrogen lines, the relative velocity in the line of sight is about* 115 *miles per second.*

This velocity has been deduced from measures of the separation of the lines at Hγ and Hδ in the photographs taken with the 6-in. prism of 45°.

(7) *The bright lines of hydrogen are superposed on broad dark bands.*

This was noted in the earliest photographs, and is fully confirmed by those subsequently obtained. Hβ, Hγ, and Hδ exhibit this feature prominently, but the same is occasionally seen at Hϵ and Hζ. It has also been noted that C presented the same characteristic when observed by Messrs. Fowler and Shackleton on October 21, 1894. The borders of these dark bands are all ill-defined, but they appear to be symmetrically placed with regard to the bright lines. The dark bands are from 10 to 12 tenth-metres broad.

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[*] 'Phil. Trans.,' A, vol. 184, p. 725.

(8) *Besides the dark bands in the positions of the hydrogen lines, there are other ill-defined dark lines.*

The additional dark lines are seen in all good negatives, whether taken with high or low dispersion, but their haziness makes their positions difficult to determine with accuracy. The approximate wave-lengths of the principal dark lines are 3887, 3968, 3994, 4008, 4025, 4069, 4101, 4119, 4143, 4168, 4340, 4388, 4471, 4643, 4860. This at once contradicts Professor Scheiner's recent statement* that he "does not believe it possible that dark lines can exist in the spectrum."

(9) *The dark lines in the spectrum of γ-Cassiopeiæ correspond very closely with the lines seen in the spectra of ζ- and γ-Orionis.*

This fact was recognised at an early stage of the inquiry, and the similarity is shown by the accompanying enlargements. The resemblance is further shown by the appended table, the wave-lengths of the lines in the spectra of ζ- and γ-Orionis being taken from a former communication.†

Dark lines in γ-Cassiopeiæ.	Dark lines in ζ-Orionis.	Dark lines in γ-Orionis.	Remarks.
3887 (5)	3887 (5)	3887 (5)	Hζ
3968 (6)	3968 (6)	3968 (6)	Hε
3994 (1)	3994 (2)	3994 (3)	
4008 (3)	4008 (2)	4008 (5)	
4025 (5)	4025 (4)	4025 (6)	
4069 (3)	4069 (2)	4069 (2)	
4101 (6)	4101 (6)	4101 (6)	Hδ
4119 (1)	—	4119 (2)	
4143 (3)	4143 (2)	4143 (5)	
4168 (2)	—	4168 (3)	
4340 (6)	4340 (6)	4340 (6)	Hγ
4388 (4)	4388 (2)	4388 (5)	
4471 (5)	4471 (5)	4471 (6)	
4643 (2)	—	4643 (2)	
4860 (6)	4860 (6)	4860 (6)	Hβ

The numbers following the wave-lengths refer to the estimated intensities of the lines, 6 representing the strongest lines.

The photographs have been taken at different times by Messrs. Fowler, Baxandall, Shackleton, North, Fournier, and Butler.

Mr. Fowler has also assisted in the preparation of the present preliminary communication.

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* 'Astronomical Spectroscopy,' Frost's translation, 1894, p. viii.

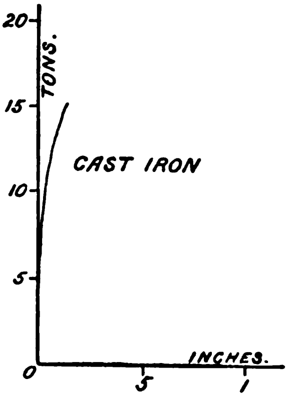
† 'Phil. Trans.,' vol. 184, A, pp. 693, 695.

II. "On the Yield Point of Iron and Steel and the Effect of repeated Straining and Annealing." By W. C. Unwin, F.R.S., Professor of Engineering, Central Technical College, City and Guilds Institute. Received December 4, 1894.

A bar is subjected to a gradually increasing tension, and the extensions for a series of loads are measured. If the loads are plotted as ordinates and the extensions as abscissæ, a *load-strain* curve is obtained. In the case of many materials the load-strain curve is fairly continuous (figs. 1 and 2). But with certain qualities of iron and steel there is a remarkable irregularity in the curve (fig. 3). Up to the elastic limit *a* the strain is proportional to the stress, and *Oa* is a straight line. Beyond *a* the strains increase faster than the stresses. At some point, *b*, termed the *yield point*, a very large and abrupt permanent elongation is produced. The bar continues to elongate for some time without increase of load, or even apparently with some decrease of load. At some point, *c*, the material is so hardened that further deformation can only be produced by increase of load, and a continuous load-strain curve, *cdf*, is obtained.

In some tests of very long iron and steel bars, by a Committee of the Institution of Civil Engineers,* the extensions for equal increments of load were carefully measured, and load-strain diagrams were plotted. These diagrams showed for the first time the abrupt

FIG. 1.



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* Experiments on the mechanical and other properties of steel, made at H.M. Dockyard, Woolwich, 1870.

Fig. 2.

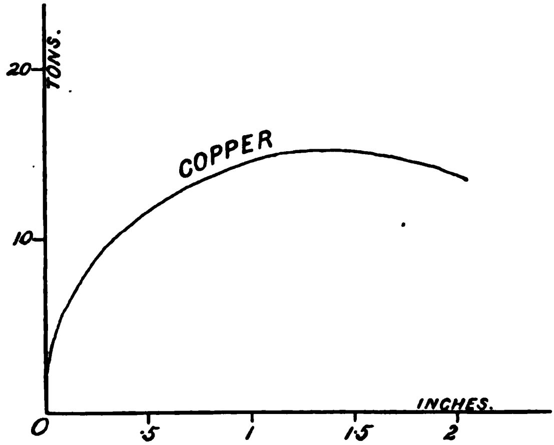
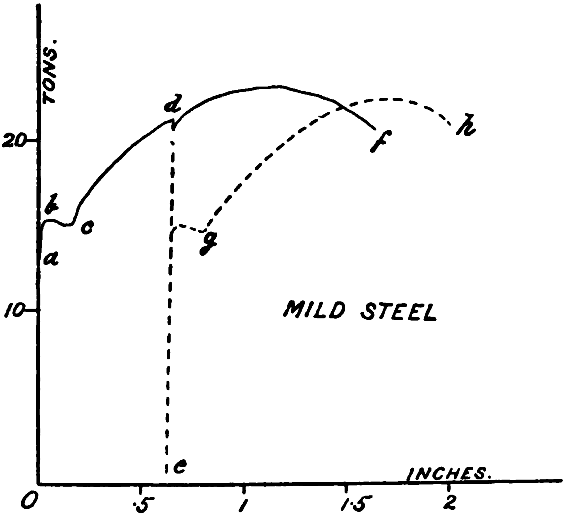


Fig. 3.

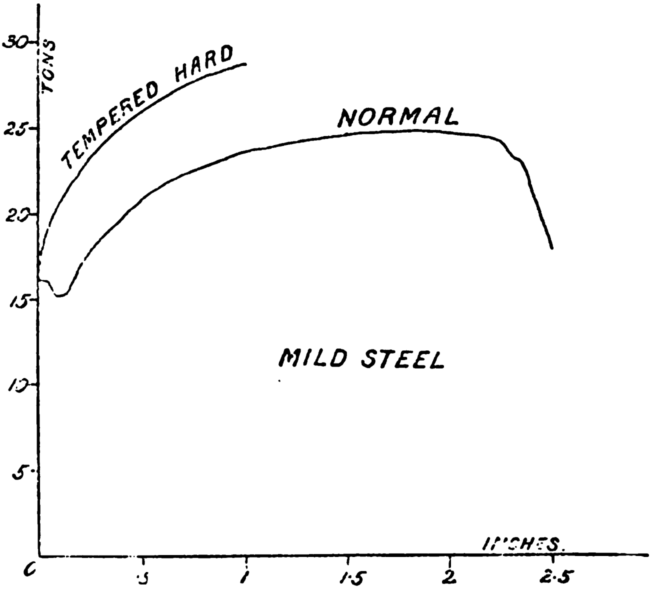


increase of extension at *b*, and the Committee gave to that point the name yielding point. It appeared from their experiments that there was a yield point also in compression, though hardly so well marked as in tension. Since these experiments, autographic apparatus has been used for describing the load-strain curve during a test. The autographic load-strain curves exhibit the yield point in the case of certain materials in a very striking way.

If at some point, *d*, the load is removed and again imposed, then in the reloading a curve, *edf*, is obtained without any marked yield point. The material is physically a different material, when it has been strained beyond the yield point. The new curve is similar to the curves figs. 1 and 2, and unlike the primitive curve *Oabcd*, fig. 3, for the material. In the case of a steel which will slightly harden if heated and suddenly cooled, the load-strain curve is *Oacdf*, if it is tested unhardened, and very similar to *edf* if it is hardened before testing.

Fig. 4 shows load-strain curves for two pieces of the same steel unhardened and tempered by heating and sudden cooling.

FIG. 4.

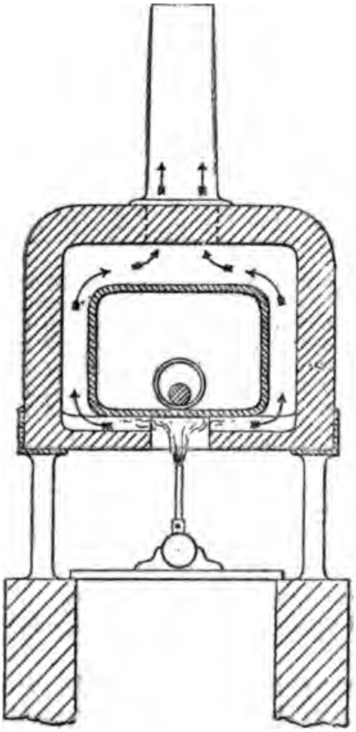


As the yield point only occurs in materials which have been rolled or hammered in a plastic state, it has been supposed to be due to some purely mechanical condition of constraint produced by pressure, which could be broken down if the stress reached the yield point. But any purely mechanical condition of constraint, such as is produced by cold hammering, is entirely removed by annealing. On the other hand, annealing the bar before testing does not alter the yield point. If anything, it makes it more distinctly marked. Further, if the bar is strained to the point *d* and the load removed, or if it is hardened by sudden cooling, and the bar then annealed and again tested, it is found to have returned to a condition almost identical with that which it had at first. In testing again, a new yield point appears, and the new load curve is such a curve as *egh*.

Osmond appears first to have suggested that in the case of iron or steel any stress which produces a permanent deformation is attended by a rearrangement of the atoms within the molecules of the metal, such as marks the passage of the iron from one state into another allotropic state.[*] The following experiments on alternate straining and annealing, in which straining appears always to produce effects like those which occur in hardening steel, and which, like them, are completely destroyed by annealing, would be most easily understood, if Osmond's view were accepted.

In the following experiments a bar was alternately tested to a stress somewhat beyond the yield point and then annealed. The annealing oven, shown in section in fig. 5, was an iron chamber with

Fig. 5.



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[*] 'Études Métallurgiques,' Paris, 1888. Also, 'Introduction to the Study of Metallurgy,' by W. C. Roberts-Austen, p. 13.

a fire-clay jacket; the chamber was heated by a row of Bunsen burners, the products of combustion passing round the annealing box. When the bar to be annealed was red-hot it was turned end for end, and shortly after the gas was extinguished, and the chamber allowed to cool down slowly with the bar in it. Every time the bar was strained beyond the yield point, its diameter sensibly decreased from lateral contraction. Some further diminution of section was due to oxidation in the annealing oven. After each annealing the bar was re-measured before re-testing. In each test an autographic load-strain diagram was taken. In the final loading the bar was broken.

Experiment I.

Mild Steel Bar, No. 1431.

Original diameter = 0·750 in. Area = 0·4418 sq. in. Clips 8 in. apart.

No. of loading.	Initial diameter at each loading.	Initial area for each loading.	Load put on, tons.	Load put on, tons per sq. in. of original section.	Load at yield point, tons.	Stress at yield point. Tons per sq. in. of		Elongation in original 8 inches.	
						Original initial section.	Reduced initial section for each loading.	For each loading	Total from beginning of test.
								in.	in.
1	0·750	0·4418	8·25	18·67	7·71	17·45	17·45	0·24	0·24
2	0·734	0·4232	8·03	18·18	7·44	16·84	17·58	0·29	0·53
3	0·716	0·4030	8·00	18·11	7·30	16·52	18·15	0·36	0·89
4	0·699	0·3834	6·99	15·82	6·58	14·89	17·16	0·31	1·20
5	0·687	0·3706	6·99	15·82	6·58	14·89	17·75	0·33	1·53
6	0·676	0·3590	6·97	15·78	6·53	14·78	18·19	0·34	1·87
7	0·665	0·3474	8·74	19·78	6·50	14·71	18·71	2·04	3·91 Broken.

In annealing, the bar was put into the furnace cold, the gas was then lit, and the bar left until it became dull red in colour. It was then changed end for end, and after a few minutes the gas was turned off, and the bar allowed to cool in the furnace.

The bar was laid upon the bottom of the furnace, and not protected in any way.

On examining the autograph diagrams (fig. 6) it will be seen that the yield point was reproduced after every annealing in as marked a way as in the first testing. Reckoning on the area at the beginning of each test, there is a rise in the stress at the yield point from 17·45 tons per sq. in. to 18·71 tons per sq. in. in seven successive loadings

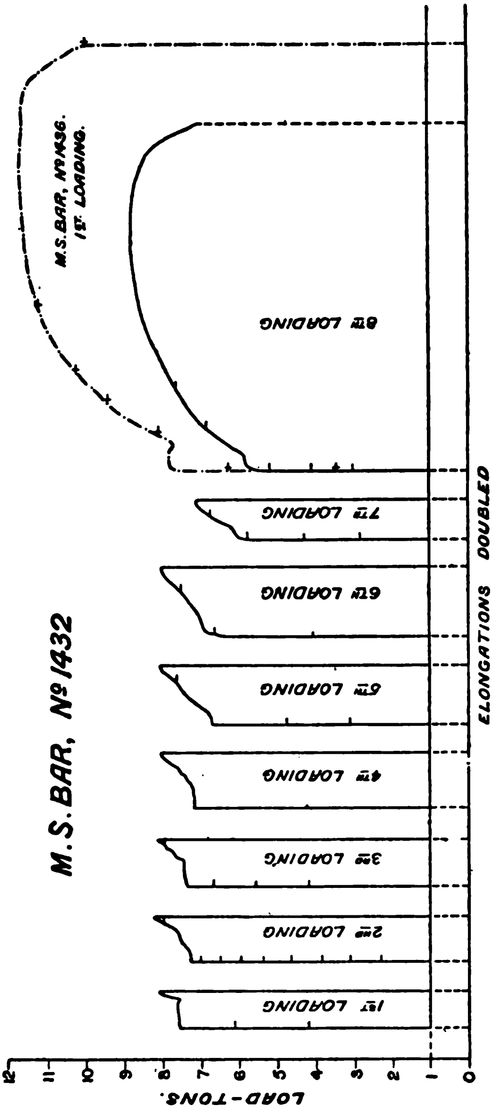


Fig. 6.

and annealings. The rise is not very considerable. During these testings the total elongation of the bar was 3·91 in. in 8 in.

The bar was not protected at all during annealing. It was put into the furnace cold, and the gas was then lit. When the bar had heated to a dull red colour, it was changed end for end, and after

Experiment II.

Wrought Iron Bar, No. 626.

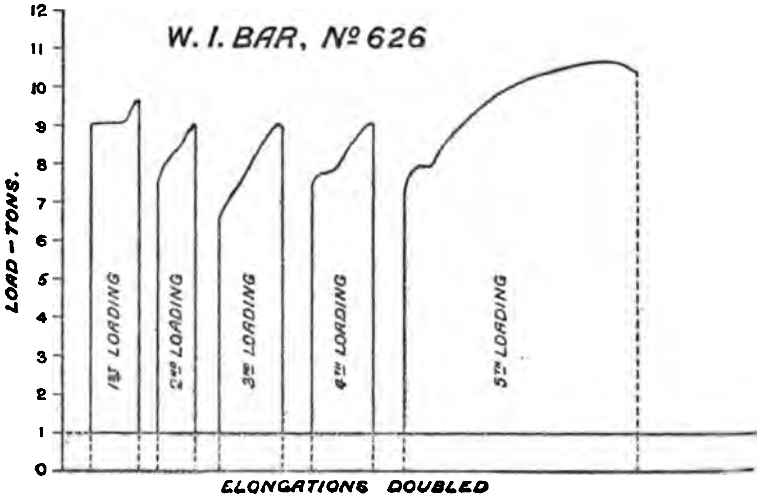
Original diameter = 0·844 in. Area = 0·5596 sq. in. Clips 8 in. apart.

No. of loading.	Initial diameter at each loading.	Initial area for each loading.	Load put on, tons.	Load put on, tons per sq. in. of original section.	Load at yield point, tons.	Stress at yield point. Tons per sq. in. of		Elongation in original 8 inches.		
						Original initial section.	Reduced initial section for each loading.	For each loading.	Total from beginning of test.	
								in.	in.	
1	0·844	0·5596	9·70	17·33	9·11	16·28	16·28	0·27	0·27	
2	0·830	0·5411	9·03	16·14	7·51	13·42	13·88	0·20	0·47	
3	0·820	0·5281	9·03	16·14	6·65	11·89	12·60	0·34	0·81	Seam opened.
4	0·806	0·5094	9·08	16·23	7·85	14·03	15·41	0·33	1·14	
5	0·793	0·4937	10·70	19·12	7·97	14·24	16·14	1·32	2·46	Broken.

two minutes the gas was turned off, and the bar allowed to cool in the furnace.

The autograph diagrams are shown in fig. 7. The yield point is generally less well marked in wrought iron than in steel. In tests 2 and 3 above, although there was rapid elongation beyond a certain

Fig. 7.



stress, there was a badly-marked yield point. The annealing may have been imperfect. The yield point reappeared more characteristically in succeeding tests after annealing. Excluding tests 2 and 3, the position of the yield point is practically constant during five loadings and annealings.

Experiment III.

Mild Steel Bar, No. 1432.

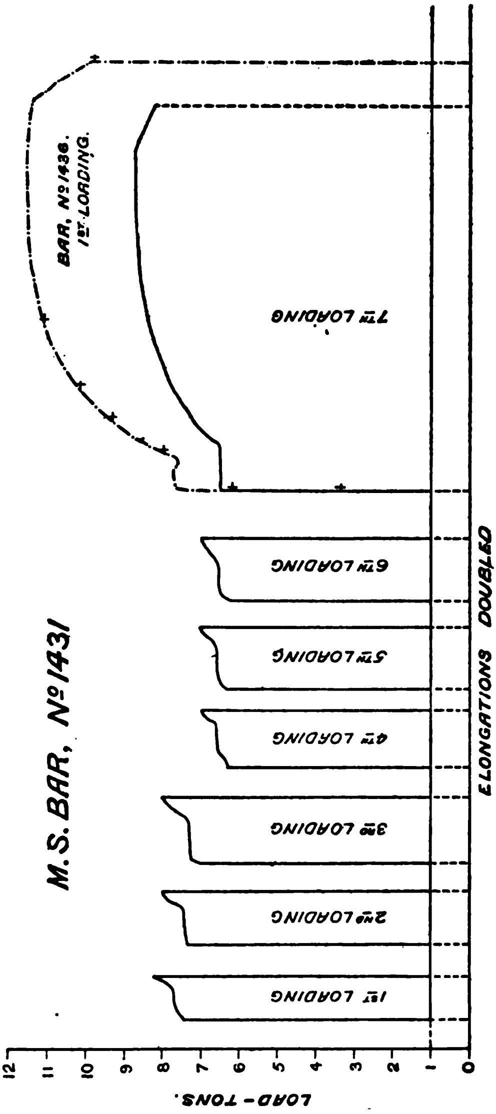
Original diameter = 0·749 in. Area = 0·4406 sq. in. Clips 8 in. apart.

No. of loading.	Initial diameter at each loading.	Initial area for each loading.	Load put on, tons.	Load put on, tons per sq. in. of original section.	Load at yield point, tons.	Stress at yield point. Tons per sq. in. of		Elongation in original 8 inches.	
						Original initial section.	Reduced initial section for each loading.	For each loading.	Total from beginning of test.
								in.	in.
1	0·749	0·4406	8·05	18·27	7·54	17·11	17·11	0·20	0·20
2	0·738	0·4278	8·30	18·84	7·25	16·45	16·95	0·24	0·44
3	0·727	0·4152	8·16	18·52	7·35	16·68	17·70	0·26	0·70
4	0·715	0·4015	8·06	18·29	7·15	16·23	17·81	0·29	0·99
5	0·702	0·3870	8·03	18·23	6·68	15·16	17·26	0·32	1·31
6	0·689	0·3728	8·01	18·18	6·88	15·61	17·78	0·39	1·70
7	0·677	0·3599	7·00	15·89	5·67	12·87	15·75	0·21	1·91
8	0·672	0·3547	8·68	19·70	5·79	13·14	16·32	1·85	3·76 Broken.

The bar was protected during annealing by placing it inside an iron tube. The furnace containing the iron tube was first allowed to get hot, the bar was then inserted, and when heated to a dull red colour it was changed end for end, the gas was turned off, and the bar allowed to cool in the furnace.

The autograph diagrams are given in fig. 8. One-minute intervals of time were marked on the diagram during the tests. The yield point in this case falls from 17·11 to 16·32 tons per sq. in. after eight tests and annealings. The difference is not great. The total elongation of the bar was 3·76 in. in eight tests.

Fig. 8.


M.S. BAR, No 1431
BAR, No 1436.
1st LOADING.
1st LOADING
2nd LOADING
3rd LOADING
4th LOADING
5th LOADING
6th LOADING
7th LOADING
ELONGATIONS DOUBLED
LOAD – TONS.
0
1
2
3
4
5
6
7
8
9
10
11
12

Experiment IV.

Mild Steel Bar, No. 1436.

Original diameter = 0·750 in. Area = 0·4418 in. Clips 8 in. apart.

Maximum load	11·56 tons	= 26·17 tons per sq. in.
Breaking load	9·84 ,,	= 22·27 ,,
Load at yield point	7·73 ,,	= 17·49 ,,
Total elongation on 8 in.	2·28 ,,	= 28·5 per cent.
Time of test up to yield point	3 minutes.	
Time of whole test	8 ,,	

In this experiment a bar of the same steel as in the previous test was broken in the first loading without any annealing, in order that its autograph diagram might be compared with that of a bar broken after eight annealings. The diagram is drawn dotted in fig. 8. There is no marked difference in the two diagrams, if the decrease of area due to successive tests and annealings is allowed for.

It is clear from these tests that the change produced in the iron or steel bar by straining beyond the yield point is completely reversed by simple annealing, and this apparently after any number of repetitions of the process. It appears hardly possible to understand either the singular abruptness of the load strain curve at the yield point, or the difference of the load strain curves for an unstrained and a strained bar, or the reversal of the change of properties produced during straining by simple annealing, except by supposing that at the yield point there occurs a chemical or molecular or allotropic change of the same kind as that which occurs in ordinary tempering by sudden cooling.

If mere stress can produce a change of this kind, it may be that the stresses produced in sudden cooling also are factors in the changes which occur in that case. If a red-hot bar is plunged into water the surface must cool first. A condition of stress must arise in which there is a ring tension and radial thrust in the section of the bar. It is well known that the stresses so set up are often of great intensity. Bars tear or crack in hardening. The material may be strained beyond its yield point by the stresses due to cooling. When bars are subjected to a Wöhler test, that is, to a large number of repetitions of straining action, they break ultimately with a stress much less than that which would break them in a simple loading and with a very characteristic fracture. The outside of the fracture is fine grained, and resembles a crack extending gradually across the section of fracture. However ductile the material, initially, there is no sign of contraction at the fracture. The bar breaks like a brittle material. It seems possible that here also the repetition of the straining action may produce a molecular change in the bar in the neighbourhood of the fracture.

III. "Preliminary Note on Embryonic Fission in *Lichenopora*." By SIDNEY F. HARMER, M.A., Fellow of King's College. Cambridge, Superintendent of the University Museum of Zoology. Communicated by A. SEDGWICK, F.R.S. Received November 3, 1894.

I have shown on a previous occasion* that the primary embryo of *Crisia* gives rise to numerous larvæ by a constantly repeated process of embryonic fission, and I have suggested the probability that this method of development will be found to be characteristic of Cyclostomatous Polyzoa in general. Observations recently made on *Lichenopora verrucaria*, Fabr., obtained on the coast of Norway,† have enabled me to prove the occurrence of embryonic fission in a second genus of Cyclostomata, although the details of the development are remarkably different from those of *Crisia*.

The colonies of *Lichenopora verrucaria* occur in great numbers on fronds of *Laminaria saccharina*, growing just beneath low-water mark. The colony is regularly plano-convex, with a diameter, in the full-grown condition, of about 5 mm.; and it is attached by its flat surface to the sea-weed. Embryonic development commences with the beginning of the formation of the colony, and the earliest stages can only be followed by examining the initial stages of the colony itself.

The growth of the young colony closely resembles that of *Tubulipora flabellaris*, Fabr., as described by Barrois.‡ The colony originates from a circular disc, which, as Barrois has shown, results from the calcification of the outer part of the body-wall of the larva after its fixation. This disc is continued into the primary zoœcium, which buds off simultaneously, on its side turned towards the sea-weed, two new zoœcia, more or less parallel with itself. *The first brood of embryos is normally developed from the egg formed in one of these two zoœcia*, although the egg is in some colonies formed in a younger zoœcium.

The colony continues to grow in the form of a compressed funnel, which at first lies on one of its flattened sides. By further growth, the mouth of the funnel soon curves round so as to look directly away from the sea-weed, its rim meanwhile extending in a plane parallel to the surface of attachment, so as to overlap and finally cover the disc from which the colony originated. The adult colony may be compared with a funnel with a basal angle of nearly 180°, passing off into a short stalk, which is curved round so as to lie between the base of the funnel and its attachment.

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* 'Quart. Journ. Mic. Sci.,' vol. 34, 1893, p. 199.

† For a part of this material I am indebted to the kindness of Dr. Brunchorst and Dr. Nordgaard, of the Bergen Museum.

‡ 'Recherches sur l'Embryologie des Bryozoaires,' 4to., Lille, 1877, pl. 4.

The funnel is composed of a multitude of closely packed pyramidal tubes, whose wider ends open at the surface in the manner of the cells of a honey-comb. These are continuously formed at the margin of the colony, where fresh tubes are constantly cut off, simultaneously with the extension of the margin, by the upgrowth of calcareous septa. The body-cavity is here closed in on the upper side by an uncalcified body-wall. The new septa are developed at the growing edge in such a way that two kinds of tubes are formed. These give rise respectively to the zoœcia and to the ovicell. The zoœcia all reach the base of the colony, where they are in close contact with one another. In growing upwards they diverge, so that a set of pyramidal spaces, narrow below and widening above, originate between them. These spaces, from which the ovicell is developed, acquire complete calcareous walls, their roof remaining at first membranous. Each contains a part of the body-cavity, cut off from the growing edge of the colony, and the spaces may accordingly be compared with zoœcia, in which, however, no polypide-buds are formed.

The membranous roof of these interzoœcial spaces becomes calcified, thus forming a "crust" which extends horizontally between adjacent zoœcia. The side walls of the spaces then break down, and a large continuous cavity thus results. This cavity, the *ovicell*, lies superficial to the bases of the zoœcia, some of which pass through it as free columns which project beyond its roof. The ovicell later acquires one or more trumpet-shaped openings to the exterior.

The mature ovicell contains several hundred larvæ in various stages of development. These, constituting the first brood, have all been derived from the primary embryo which normally develops in one of the two zoœcia first formed from the primary zoœcium.

The beginning of the developmental processes was found in a colony consisting of three zoœcia only. Each of these contained a "brown body" and a mature polypide. The polypide of the primary zoœcium, like that of one of the two secondary zoœcia, possessed a testis, attached to the lower end of its alimentary canal. That of the remaining zoœcium had an egg in the corresponding position.

A precisely similar egg was found, in one or two young colonies, in a polypide-bud, where, from its position, it had probably been developed from the outer (mesodermic) layer of the bud. Eggs are not necessarily confined to a single polypide in the colony, but in a condition in which they can be recognised as eggs, they do not appear to be developed in most cases except in the polypide destined to give rise to a primary embryo. The testis, on the contrary, may be regarded as a normal possession of every polypide. It usually gives rise to an enormous number of spermatozoa. Fertilisation has not been observed, although a testis may be developed by a polypide which carries an egg, and although free, ripe spermatozoa have been noticed in the immediate neighbourhood of young primary embryos.

The primary embryo in its older stages is always in the immediate neighbourhood of a "brown body," a structure which was found, with rare exceptions, in the younger stages as well. Unless it should hereafter appear that a "brown body" may be formed by the degeneration of a part of a polypide, the presence of this structure in a zoœcium containing an egg or a very young embryo clearly indicates that the egg itself belongs to the second polypide which tenants the zoœcium.

Every colony normally gives rise to an ovicell, although cases are not uncommon in which degeneration sets in at an earlier or later period, resulting in the atrophy of the embryonic tissues or in their failure to develop further.

Development of the colony and of the egg go on *pari passu.* The details of the segmentation of the egg are even more difficult to observe than in *Crisia;* but stages were found in which the number of embryonic nuclei progressively increases. By the time that the embryo comes to consist of four or five cells, it is surrounded by other cells which form the commencement of a follicle. This structure is at first composed of loosely-arranged cells, hardly distinguishable from the "funicular tissue" which occurs in the body-cavity generally. The follicle becomes more compact, and finally assumes a form which has a striking similarity to a stage described by Kraepelin* in the early development of *Plumatella.* In this condition the embryo consists of a small spherical mass of protoplasm, including a few nuclei. It is supported by a structure resembling the suspensor which supports the embryo in flowering plants. This suspensor contains a very fine lumen, and a section which passes longitudinally through its axis accordingly shows two rows of nuclei, one row belonging to each of the walls on opposite sides of the lumen. The suspensor and the embryo are surrounded by a common investment of cells; and the whole arrangement is attached either to the testis of the polypide or to the lower end of the alimentary canal when no testis is present. The embryo, with its investment, hangs down freely into the body-cavity, and it is always in close connexion with the "brown body" to which allusion has already been made.

Kraepelin and Braem† are agreed in deriving the corresponding structure in Phylactolæmata from a rudimentary polypide-bud, the part which I have termed the suspensor corresponding with the inner layer of the bud, and the outer investment with its outer layer. Although this interpretation would make the agreement between *Lichenopora* and the Phylactolæmata closer than would otherwise be the case, I have not succeeded in showing that the arrangement has really the morphological value of a polypide-bud.

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* 'Abhandl. Naturwiss. Ver. Hamburg,' vol. 12, 1893 (Bryozoen), pl. 2, figs. 67, 68.

† Leuckart and Chun's 'Bibliotheca Zoologica,' vol. 2, part 6, 1890; see explanation of fig. 171 (pl. 15).

The polypide now begins to pass into the condition of a new "brown body," with which the old "brown body" fuses. The embryo, with its investments, hangs down into the zoœcium from the wall of the "brown body," which has meanwhile moved up the zoœcium to the neighbourhood of the orifice. In this position it remains during the later stages, becoming much complicated by the processes which succeed.

The suspensor ceases to be sharply defined as such, its cells merging with those which surround the "brown body" and the embryo. The latter comes to lie close to the "brown body;" and after a large increase in the number of its nuclei, begins to undergo fission. But whilst in *Crisia* the primary embryo remains throughout in the condition of a coherent mass of cells, which buds off successive embryos into the ovicell, that of *Lichenopora* early divides up into a considerable number of irregular pieces, which ultimately give rise to the secondary embryo. In the earlier stages of the process it is easy to show that the pieces are continuous with one another, or that, from their position and structure, they have recently been separated from one another. In older ovicells it may be difficult to demonstrate the existence of the process of fission, since the number of embryos is increased, to a considerable extent at least, by the binary division of small masses of cells which have all the appearance of young embryos. The details of the fission are, in fact, more similar to those which occur in the fission of the "Urknospen" of certain Tunicata (*Doliolum*,* &c.) than to those which occur in *Crisia*.

After the first brood of larvæ are hatched the colony may develop new primary embryos at several points in the ovicell. These form the starting points for the development of a fresh brood of larvæ. Thus, early stages in the embryonic development may be found, not only in the youngest colonies, but also in the largest, fully-formed colonies. These latter differ from the young colonies in containing the set of cavities constituting the original ovicell. The primary embryos of the second generation may readily be recognised in sections by means of the characteristic "brown body," to which the follicle containing the embryo is attached. Their earliest stages have not so far been traced; but the later stages, in which fission is commencing, are, in all important points, identical with those of the first generation.

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* Uljanin (B.), 'Fauna und Flora G. von Neapel,' X Monogr., 1884, pp. 108 et seq.

After the commencement of fission the follicle may contain numerous egg-like cells, which are probably identical with the giant cells which I have previously described in the ovicell of *Crisia.* Although it is not impossible that these cells may play some part in the formation of the secondary embryos, this is rendered improbable, not only by the analogy of *Crisia*, but still more by the fact that the early stages in the development of the primary embryo take place before such cells can be recognised in the ovicell.

The following results may be deduced from the preceding statements:—

1. The ovicell is not altogether external to the zoœcia, as might be inferred from some of the older descriptions of this structure. Its cavity is morphologically identical with the body-cavity of the zoœcia, and the ovicell results from the breaking down of numerous septa which at first separate from one another a set of tubes formed at the growing edge of the colony in the same manner as the ordinary zoœcia.
2. The development of the ovicell and that of the embryo normally commence at almost the beginning of the life of the colony. So long as the growth of the first brood of larvæ continues there is no development of new primary embryos; and the numerous young larvæ found in the ovicell are descendants of the single primary embryo which is normally produced in one of the two zoœcia first budded off from the primary zoœcium.
3. The process cannot well be interpreted as a form of alternation of generations. A large number, perhaps the great majority, of the secondary embryos are formed by the direct fission of pre-existing embryos, and are not budded off from a compact mass of cells as in *Crisia.*
4. Certain remarkable analogies may be detected between the development of the Cyclostomata and that of the Phylactolæmata. Further research will be necessary in order to show whether these resemblances are more than mere analogies.

IV. "The Influence of the Force of Gravity on the Circulation." By LEONARD HILL, M.B., Assistant Professor of Physiology, University College, London, and Grocers' Research Scholar. Communicated by Professor SCHÄFER, F.R.S. Received November 19, 1894.

(From the Physiological Laboratory of University College, London.)

(Abstract.)

The effect of position of the body upon the circulation of the blood is a matter of daily observation with the physician and surgeon, but

it has been curiously neglected by physiologists. So far as my researches into the history of the subject go, the mere fact that the feet-down position lowers arterial pressure, and that the feet-up position heightens it, is almost all that has been determined.

In 1885, Hermann placed the subject in the hands of two pupils, Blumberg and Wagner, with the object of investigating the dynamic and hydrostatic effects of gravity on the circulation.

The hydrostatic moment is the altered relationship of level between the given spot on the artery (where the manometer is placed) and the rest of the vascular system.

The dynamic moment is the altered relation between pressure and resistance produced indirectly by the change of position on the heart-beat, the filling of the heart, the vaso-motor nerves, &c.

Hermann instructed his pupils to find the indifferent point of the circulatory system, in order that the hydrostatic effect might be eliminated and the dynamic effect alone studied.

Blumberg and Wagner determined the indifferent point by filling the circulatory system of the dead animal with salt solution and then by shifting the position of the axis round which the body was turned.

I have found that on attempting, according to Wagner's method, to fill the circulatory system of the dead animal with salt solution, the arterial part of the system does not remain filled, for the salt solution rapidly leaks into the splanchnic venous area. By alternately placing the animal with feet down or with feet up, a pumping action is produced which gradually forces the salt solution out of the arterial system into the venous side, where it remains. The indifferent point cannot, therefore, be found on the dead body. Further, if it were possible to find the indifferent point on the dead body, the indifferent point on the living body could not be deduced therefrom, because such indifferent point depends on the coefficient of elasticity which must constantly alter in the living animal with every alteration of the arterioles by vaso-constriction or dilatation.

As regards the dynamic effect of gravity on the circulation, therefore, the work of Blumberg and Wagner cannot be accepted.

My attention was first drawn to the influence of gravity upon the circulation by observations which I made upon the normal intra-cranial tension in a patient of Dr. Claze-Shaw. This patient had been trephined, and Dr. Shaw asked me if I could estimate his normal intra-cranial pressure. I did so by an adaptation of the method for investigating intra-cranial pressure described by me in the 'Proceedings of the Royal Society,' vol. 55.

I found that the pressure was negative while the man sat upright, but that it became positive as soon as the head was bent down towards the knees, and on any expiratory effort. The air-bubble index in the apparatus exhibited large cardiac and respiratory undulations.

Experimenting on dogs, I found that exactly the same thing occurred. The normal cerebral pressure became markedly negative in the feet-down posture, and positive in the feet-up posture.

For the further investigation of the subject, I constructed an animal holder which could be swung round a horizontal axis. In this axis the cannula connected with the vessel under observation was always placed, and the cannula itself was connected with a fixed hydrostatic manometer.

The "hydrostatic and dynamic moments," to use Hermann's expression, were investigated and separated, not by attempting to find the indifferent point, but by carefully observing the effects of dividing and stimulating the vagus and splanchnic nerves and spinal cord, and by watching the influence of anæsthetics, curare, and asphyxia.

The research has been carried out upon rabbits, cats, dogs, and monkeys, and the same general results have been obtained from all. The animals were anæsthetised in all the experiments, and were placed upon a board with the limbs fully extended in the same direction as the longitudinal axis of the body.

The experiments group themselves under the following headings:

A. Effects on the Circulation.

i. Normal effect on arterial pressure.
    (*a*) With carotid artery in axis.
    (*b*) With femoral artery in axis.
    (*c*) With splenic artery in axis.
ii. Normal effect on venous pressure.
    (*a*) With splenic vein in axis.
    (*b*) With femoral vein in axis.
    (*c*) With torcula Herophili in axis.
iii. Influence of anæsthetics.
iv. Effect of dividing the vagi.
v. Effect of dividing the spinal cord.
    (*a*) Influence on heart.
vi. Effect of dividing the splanchnics.
vii. Influence of respiration and asphyxia.
viii. Influence of curare.

B. Effects on Respiration.

i. Normal effects.
ii. Effect of dividing the vagi.

The venous pressures were recorded by means of a manometer filled with sat. $MgSO_4$ sol. and placed in connection with a delicate tambour or piston recorder. The cerebral venous pressure was taken in the torcula Herophili by the method described by me in the before-quoted paper.

The respiratory tracings were taken by means of a broad band of strapping passed round the thorax and connected to either side of a Paul Bert tambour. The changes of position prevented the use of any more accurate method for recording the respiration.

The results of the research are shown in a series of tracings, from which the following conclusions are drawn:—

1. That the force of gravity must be regarded as a cardinal factor in dealing with the circulation of the blood.
2. That the important duty of compensating for the simple hydrostatic effects of gravity in changes of position must be ascribed to the splanchnic vaso-motor mechanism.
3. That the effects of changing the position afford a most delicate test of the condition of the vaso-motor mechanism.
4. That the amount of compensation depends largely on individual differences.
5. That the compensation is far more complete in upright animals such as the monkey, than in rabbits, cats, or dogs, and, therefore, is probably far more complete in man.
6. That in some normal monkeys over-compensation for the hydrostatic effect occurs.
7. That in the normal monkey and man gravity exerts but little disturbing influence, owing to the perfection of the compensatory mechanism.
8. That when the power of compensation is damaged by paralysis of the splanchnic vaso-constrictors, induced by severe operative procedures or by injuries to the spinal cord, by asphyxia, or by some poison such as chloroform or curare, then the influence of gravity becomes of vital importance.
9. That the feet-down position is of far greater moment than the feet-up position, because when the power of compensation is destroyed the blood drains into the abdominal veins, the heart empties, and the cerebral circulation ceases.
10. That, generally speaking, the feet-up position occasions no ill consequence.
11. That the horizontal and feet-up positions at once abolish the syncope induced by the feet-down position by causing the force of gravity to act in the same sense as the heart, and thus the cerebral circulation is renewed.
12. That firmly bandaging the abdomen has the same effect. While the heart remains normal, and so long as the mechanical pressure is applied to the abdominal veins, the blood pressure cannot possibly fall.

13. That if the heart is affected, as by chloroform or curare poisoning, the restoration of pressure is incomplete, and it is possible that the heart may be stopped altogether by the inrush of a large quantity of blood, caused by too rapid an application of pressure on the abdomen. More work would be thrown upon the heart than, in its impoverished condition, it could perform.
14. That vagus inhibition and cardiac acceleration are subsidiary compensatory mechanisms in the feet-up and feet-down positions respectively.
15. That chloroform rapidly paralyses the compensatory vasomotor mechanism, and damages the heart.
16. That ether, on the other hand, only paralyses the compensatory vaso-motor mechanism very slowly and when given in enormous amounts.
17. That the vaso-motor paralysis induced by these anæsthetics lasts for some considerable time after the removal of the anæsthetics.
18. That chloroform can, by destroying the compensation for gravity, kill the animal, if it be placed with the abdomen on a lower level than the heart.
19. That elevation or compression of the abdomen immediately compensates for the vaso-motor paralysis produced by chloroform.
20. That compression or elevation of the abdomen, coupled with artificial respiration and with squeezing of the heart through the thoracic walls, is the best means of restoring an animal from the condition of chloroform collapse. That these results agree entirely with McWilliams', and are opposed to those of the Hyderabad Commission.
21. That the feet-down position inhibits respiration, and the feet-up position accelerates it.
22. That these respiratory results probably depend upon the stimulation of sensory nerve endings by changes of tension brought about by the alterations of position, because the results are abolished by dividing the vagi.
23. That in the feet-down position the respiration is thoracic in type, and the abdomen is retracted; in the feet-up position the respiration is diaphragmatic and the abdomen freely expanded.
24. That these types of respiration tend to compensate for the effects of gravity on the circulation, for the retraction of the abdomen in the feet-down position mechanically supports the abdominal veins, whilst the thoracic inspirations aspirate blood into the heart. In the feet-up position the full and free expansion of the abdomen withdraws all obstacles to the compensatory dilatation of the abdominal veins.

In the last part of the paper the medical aspects of this research are discussed. It is suggested that emotional syncope is due to paralysis of the splanchnic area, and a case is quoted where compression of the abdomen immediately removed the syncopal condition. The same treatment, or that of elevation of the abdomen, is suggested for conditions of shock, chloroform collapse, and after severe hæmorrhage.

Finally, a parallel is drawn between some of the results of this research in reference to monkeys and those obtained by Dr. George Oliver on man, by measuring the diameter of the radial artery with his ingenious instrument, the arteriometer.

The Chairman announced that a paper on a newly-discovered gas having been promised by Lord Rayleigh and Professor Ramsay, this paper would on January 31 be taken as a subject for discussion under a Resolution of Council passed last session, whereby in each year certain Ordinary Meetings were to be "devoted each to the hearing and consideration of some one important communication, or to the discussion of some important topic."

The Society adjourned over the Christmas Recess to Thursday January 17, 1895.

Presents, December 13, 1894.

Transactions.

Buitenzorg:—'s Lands Plantentuin. Mededeelingen. Nos. 11, 12. 8vo. *Batavia* 1894. The Director.

Cambridge:—Cambridge Philosophical Society. Proceedings Vol. VIII. Part 3. 8vo. *Cambridge* 1894. The Society.

Cordova:—Academia Nacional de Ciencias. Boletin. Tomo XIII. Entr. 3, 4. 8vo. *Buenos Aires* 1893. The Academy.

Göttingen:—Königl. Gesellschaft der Wissenschaften. Nachrichten (Philol.-hist. Classe). 1894. No. 3. 8vo. *Göttingen.* The Society.

Helsingfors:—Finska Vetenskaps Societet. Acta. Tom. XIX. 4to. *Helsingforsiæ* 1893; Bidrag till Kännedom af Finlands Natur och Folk. Häft. 52, 53. 8vo. *Helsingfors* 1893; Öfversigt af Förhandlingar. Häft. 35. 8vo. *Helsingfors* 1893. The Society.

Kew:—Royal Gardens. Bulletin of Miscellaneous Information. 1894. Appendix 3. 1895. Appendix 1. 8vo. *London* 1894–95. The Director.

Transactions (*continued*).

London:—Geologists' Association. Proceedings. Vol. XIII. Part 10. 8vo. *London* 1894. The Association.

Royal Photographic Society. Journal and Transactions. Vol. XIX. No. 3. 8vo. *London* 1894. The Society.

Society of Biblical Archæology. Proceedings. Vol. XVI. Part 9. 8vo. *London* 1894. The Society.

Paris:—Société Entomologique. Annales. Vol. LXIII. Trim. 2, 3. 8vo. *Paris* 1894. The Society.

Stockholm:—K. Svenska Vetenskaps Akademie. Handlingar. Bd. XXV. Häft 2. 4to. *Stockholm* 1893–94. The Academy.

Toulouse:—Faculté des Sciences. Annales. Tome VIII. Fasc. 4. 4to. *Paris* 1894. The Faculty.

Warwick:—Warwickshire Naturalists' and Archæologists' Field Club. Annual Report. 1892–93. 8vo. *Warwick.* The Club.

Observations and Reports.

Paris:—Bureau des Longitudes. Éphémérides des Étoiles de Culmination Lunaire et de Longitude. 4to. *Paris* 1894. The Bureau.

Portugal:—Direction des Travaux Géologiques. Flore Fossile du Portugal: Nouvelles Contributions à la Flore Mésozoïque, par le Marquis de Saporta. 4to. *Lisbonne* 1894. The Direction.

St. Petersburg:—Comité Géologique. Bulletins. Vol. XII. Nos. 3—7. [*Russian.*] 8vo. *St. Petersburg* 1893; Bibliothèque Géologique de la Russie. 1892. 8vo. *St. Petersburg* 1893. The Committee.

Sydney:—Observatory. Meteorological Observations. June, 1894. 8vo. [*Sydney.*] The Observatory.

Vienna:—K. K. Central-Anstalt für Meteorologie und Erdmagnetismus. Jahrbücher. Jahrg. 1892. 4to. *Wien* 1894. The Institute.

Wellington, N.Z.:—Registrar-General's Office. New Zealand Official Year-book. 1894. 8vo. *Wellington, N.Z.* The Registrar-General.

Journals.

Annaes de Sciencias Naturaes. Anno. I. No. 4. 8vo. *Porto* 1894. The Editor.

Morphologisches Jahrbuch. Bd. XXII. Heft 1. 8vo. *Leipzig* 1894. Prof. C. Gegenbaur, For. Mem. R.S.

Etheridge, R., Jun., and Jack, R. L. Catalogue of Works, Papers, Reports, and Maps on the Geology, Palæontology, Mineralogy, Mining, and Metallurgy, &c., of the Australian Continent and Tasmania. 8vo. *London* 1891. Prof. Trail, F.R.S.

Gowers (W. R.), F.R.S. The Dynamics of Life: an Address delivered before the Medical Society of Manchester. 8vo. *London* 1894. The Author.

"Observations of Sun-spot Spectra, 1879—1894." By J. Norman Lockyer, C.B., F.R.S. Received November 15, —Read November 22, 1894.

I. *The Widening of Iron Lines and of Unknown Lines in relation to the Sun-spot Period.*

It is now twenty-eight years since I discovered that the lines seen in sun-spots were subject to widening,* and that different lines were widened at different times.

It was not, however, till 1879 that I was enabled to commence daily routine work of such a nature that all observations were comparable *inter se.* This desideratum was secured by limiting attention to the twelve lines most widened between F and D.

In 1886† I gave an account of some of the early results obtained by this research. I have recently commenced the complete discussion of the whole series of observations to the present year.

This discussion, involving 21,000 lines widened during the period in question, has necessitated three special researches: the first, dealing with the lines with which, contemporaneously, coincidences have been found in the laboratory; the second, dealing with those the origin of which is so far unknown; and the third, with the distribution of both sets of lines in spots in relation to the sun-spot period.

To make the work as definite as possible, I am, in the first instance, confining the inquiry concerning the known lines to lines of iron based upon the examination of the pure electrolytic iron referred to in a previous communication.‡

The following statistics will show the relation of these iron lines to the Fraunhofer lines in the region F—D over which the spot work extends. In the table, "terrestrial line" means a line which has been photographically recorded by myself or my assistants in the spectrum of some metal or another during the past twenty-four years; "unknown" means a line not so far traced by me in any metal with the exception of Cerium. This exception is necessitated by the fact that the spectrum of that metal contains practically as many lines as appear in the solar spectrum. The wave-length map of Rowland's second series has been taken as a standard.

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* 'Roy. Soc. Proc.,' vol. 15, p. 256, 1866.

† *Ibid.*, vol. 40, p. 347.

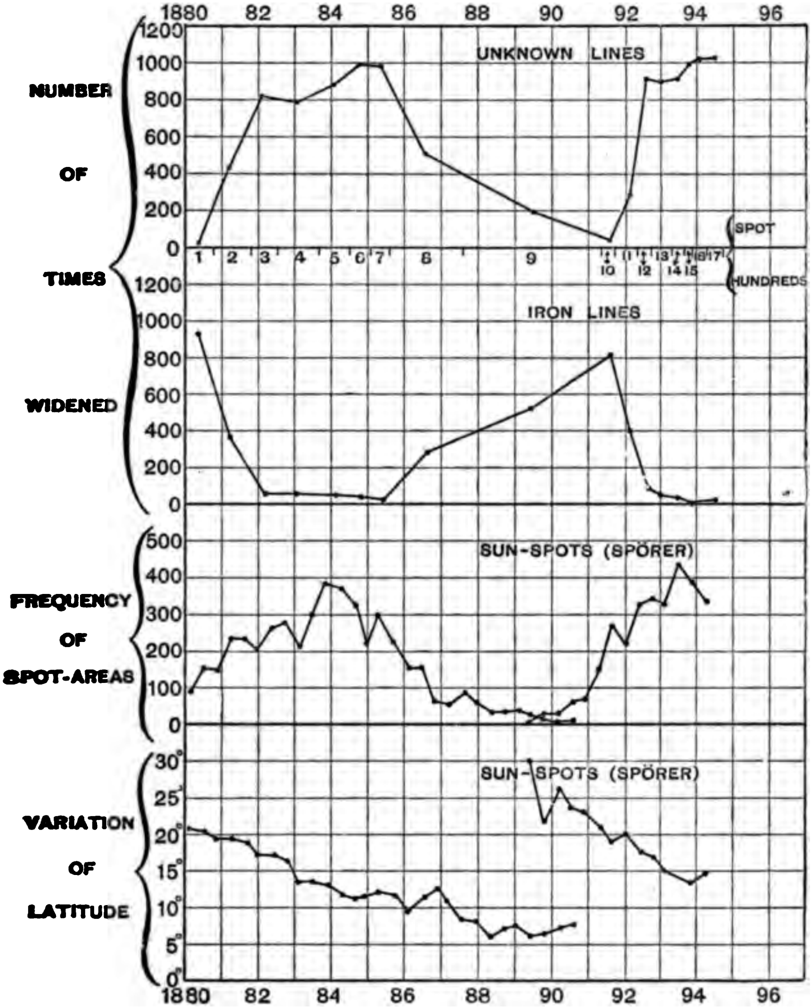
‡ *Ibid.*, vol. 54, p. 359.

Region.	Fraunhofer lines.	Terrestrial.	Unknown.	Iron.
F				
4861—4900	92	41	51	16
4900—5000	175	96	79	45
5000—5100	228	92	136	33
5100—5200	176	92	84	39
5200—5300	165	83	82	32
5300—5400	211	76	135	29
5400—5500	216	63	153	26
5500—5600	186	57	129	31
5600—5700	149	73	76	29
5700—5800	198	48	150	22
5800—5895	208	31	177	5
D				
	2004	752	1252	307

In the present communication I confine myself to submitting provisional curves based upon a preliminary inquiry into the number of times the lines of both categories have been observed to be widened in spots. Some slight corrections will, doubtless, be ultimately required when some uncertainties connected with some of the earlier observations, made before Rowland's maps were available, have been cleared up. The highest points of the curves represent the maximum frequency of iron lines in one case and of unknown lines in the other.

The period embraced by the observations practically enables us to study what has taken place at two successive sun-spot minima and two maxima. It will be seen that the phenomena which followed the minimum of 1879 have been exactly reproduced after the minimum of 1890. At the minima the iron lines are prominent among the most widened lines; at the maxima we only find lines about which nothing is known. Since the discussion indicates that the iron lines involved, which ultimately disappear, are almost invariably those seen most prominent in the spark, the view put forward in my paper of 1886 that the change observed is due to the dissociation of iron in the spots as a sun-spot maximum is approached, is corroborated, and, so far, I have heard of no other simple and sufficient explanation.

It will be noted that the maxima and minima of solar temperature thus revealed to us, if my hypothesis be confirmed, lag behind the spot maxima and minima. This may explain the lag observed in



those meteorological conditions, the secular changes in which have been held by Balfour Stewart, Broun, Meldrum, Blanford, Chambers, and others, to prove that the disturbances and changes in our own atmosphere are affected by those taking place in the atmosphere of the sun.

January 17, 1895.

The LORD KELVIN, D.C.L., LL.D., President, followed by Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Trusts of the Royal Society." By Sir JOHN EVANS, K.C.B., Treasurer R.S. Received January 7, 1895.

It may be of interest to the Fellows if I give a succinct account of the various trusts that the Society administers, their origin and progress, the application of the income of the funds, and their present financial position. I will take them in the order in which they are arranged on our annual balance sheet, on which in each case full particulars of capital, income, and expenditure, are clearly given.

A considerably larger amount of detail as to the foundation of the older funds will be found in Weld's History of the Royal Society and in an anniversary address delivered by the late Mr. Spottiswoode, as treasurer, in 1874. On the present occasion I propose to treat the subject as briefly as possible with due regard to clearness.

No. 1. THE SCIENTIFIC RELIEF FUND.

This most excellent and valuable institution originated in the year 1859, in a proposition of Mr. J. P. Gassiot that a fund of this kind should be founded for the aid of such scientific men, or their families, as may from time to time require assistance. A circular announcing the project was issued by the Society, in May, 1859, and by the following July £2500 had been subscribed. By the end of 1864, the amount of £5000 had been reached, and by 1868 the fund consisted of £6052 17*s*. 8*d*., New 3 per cent. Annuities.

Until 1884 the amount remained stationary at a little under £6500, but legacies, from the late Sir W. Siemens, of £1000, and from Mr. Bentham, of about £500, gave it a sensible help.

In November, 1885, however, a noble offer was made by Sir William, now Lord Armstrong, to present a sum of about £6500 to the fund on the understanding that an equal sum should be raised by subscription, and further that the fund might be used in cases of urgent necessity for the remission of the fees of Fellows.

Although by the beginning of 1887 it was not found possible to raise more than about £4200 by subscription, Lord Armstrong, in February, 1887, munificiently increased his contribution to £7800, so that in all £12,000 were added to the fund. Since that date about £800 more have accrued from bequests from Mr. H. B. Brady and others, and from donations. The permanent invested capital of the fund now consists of

£6000 London and North-Western Railway 4 per cent. Guaranteed Stock;

£7200 Great Northern Railway 3 per cent. Debenture Stock; and

£4340 South Eastern Railway 5 per cent. Debenture Stock;

producing an income of about £670 and of the saleable value of upwards of £24,000.

The grants last year were unusually high, amounting to £982 5*s.*; but the available balance in hand is still over £500.

The administration of the fund is entrusted to a committee consisting of ten members of whom two retire annually, and applications for grants are made through the Presidents of nine chartered scientific societies, including the Royal Society of Edinburgh and the Royal Irish Academy, who may recommend the cases for consideration by the Committee. It has not of late years been the custom fo any officer of the Society to be a member of this Committee.

No. 2. The Donation Fund.

In 1828 the late William Hyde Wollaston, M.D., at one time President of the Society, gave the sum of £2000 3 per cent. Consols, to be called the Donation Fund, the dividends to be applied from time to time in promoting experimental researches, or in rewarding those by whom such researches may have been made, or in such other manner as shall appear to the President and Council for the time being most conducive to the interests of the Society in particular, or of science in general. The application of the funds extends to individuals of all countries, but not to members of the Council of the Society. The dividends are not to be hoarded parsimoniously, but expended liberally. About £1400 more were contributed to the fund by Dr. Davies Gilbert (who gave £1000) and others. In 1874 and 1875 two legacies of £500 each were received from Sir Francis Ronalds, F.R.S., and Sir Charles Wheatstone, F.R.S., and the fund for some years consisted of £6339 Consols, which in 1888 was converted into £5030 Great Northern Railway Perpetual 4 per cent. Guaranteed Stock, producing an income of about £200 per annum. In the year 1879 the late Sir Walter C. Trevelyan bequeathed a sum of £1500, the interest to be applied in the promotion of scientific research. This was invested in the purchase of £1396 Great Northern Railway 4 per cent. Debenture Stock, now converted into £1861 6*s.* 8*d.* 3 per cent. Debenture Stock, and forms practically a part of the Donation Fund. The interest of the Jodrell Fund is also transferred to it, so that the annual income is about £390. The grants last year, however, amounted to £665, though they left a balance of £364 5*s.* 4*d.* in hand.

No. 3. The Rumford Fund.

Count Rumford, in a letter to Sir Joseph Banks dated 12th July, 1796, informed him, as President of the Society, that he had purchased and transferred £1000 Stock in the funds of this country, to the end that the interest of the same should be given once every second year as a premium to the author of the most important discovery or useful improvement which shall be made or published by printing, or in any way made known to the public in any part of Europe during the preceding two years on heat or on light, the preference always being given to such discoveries as shall, in the opinion of the President and Council, tend most to promote the good of mankind. The premium is to take the form of two medals, the one of gold and the other of silver, to be together of the value of two years' interest on the £1000, or £60 sterling. In case of there being no new discovery in heat or light during any term of years which, in the opinion of the President and Council, is of sufficient importance to deserve the premium, direction is given to invest its value in the purchase of additional stock in the English Funds, and the interest of this additional capital is to be given in money, with the two medals, at each succeeding adjudication. In a subsequent letter, Count Rumford suggests that the premium should be limited to new discoveries tending to improve the theories of fire, of heat, of light, and of colours, and to new inventions and contrivances by which the generation, and preservation, and management of heat and of light may be facilitated. Chemical discoveries and improvements in optics, so far as they answer any of these conditions, are to be within the limits of the premium, but the Count wishes especially to encourage such practical improvements in the management of heat and light as tend directly and powerfully to increase the enjoyments and comforts of life, especially in the lower and more numerous classes of society. The first recipient of the medals was Count Rumford himself. Previously to 1846 it was not unfrequently the case that no medal was adjudicated for four years. Indeed between 1818 and 1832 the only recipient was M. Fresnel. As a consequence the invested funds have increased to £2330, but the interest is now only 2¾ per cent., and will in 1903 be only 2½ per cent. A bonus on the conversion of the Stock, including an additional quarter's interest, and some returned income tax, either have been, or immediately will be, added to capital. The annual income is at present about £64, and the sum of money that accompanies the medal about £68. In adjudicating this and other medals the following procedure is adopted. At a specified meeting of the Council members are invited to suggest a name or names thought worthy of consideration. At a subsequent meeting, held before the midsummer recess, names are formally proposed and carefully prepared statements of the claims on which the proposals are based are circulated among the members of the Council. At a Council meeting in October these claims are discussed, and finally, at a meeting in November, the medals are adjudged.

No. 4. BAKERIAN AND COPLEY MEDAL FUND.

There has for many years been only one amalgamated fund for these two objects. It now consists of £403 9*s.* 8*d.* New 2½ per cent. Consols. The Bakerian Lecture originated in 1775, through a bequest of Mr. Henry Baker, F.R.S., of £100, for an oration or discourse, to be spoken or read yearly by some one of the Fellows of the Society, on such part of natural history or experimental philosophy, at such time and in such manner as the President and Council of the Society for the time being shall please to order and appoint. In case no lecture be given, there is a pain of forfeiture attached to the bequest. The payment to the lecturer has for many years been a fixed sum of £4.

The Copley Medal, which has long been regarded as the highest scientific distinction that the Royal Society can bestow, originated in a legacy of £100 from Sir Godfrey Copley, Bart., F.R.S., received in 1709. The testator directed that this sum should be laid out in experiments or otherwise for the benefit of the Society, as they shall direct and appoint. For many years the interest of the fund was paid to Dr. Desaguliers, Curator to the Society, for various experiments made before them, but in 1736 Martin Folkes, who subsequently became President of the Society, proposed to render Sir Godfrey Copley's donation more beneficial than at that time it was. His suggestion was that instead of the annual experiment, "a medal or other honorary prize should be bestowed on the person whose experiment should be best approved, by which means he apprehended a laudable emulation might be excited among men of genius to try their invention, who in all probability may never be moved for the sake of lucre." Eventually, in 1736, it was resolved that a medal of the value of £5, to bear the arms of the Society, should be awarded to the author of the most important scientific discovery or contribution to science by experiment or otherwise. The weight of the medal was fixed at 1 oz. 2 dwts. of fine gold. In 1831 is was resolved that the Copley Medal shall be awarded to the living author of such philosophical research, either published or communicated to the Society, as may appear to the Council to be deserving of that honour. The particulars of the subject of the research are to be specified in the award, and there is to be no limitation as to the period when the research was made, or the country to which the author may belong.

Owing to the payments for the lecture and the medal being somewhat less than the dividends received, a balance has gradually accumulated in favour of the fund, amounting to over £100. This, however, is now being gradually reduced, for in the year 1881, Sir Joseph Copley, Bart., transferred to the Society a sum of £1666 13*s*. 4*d*. 3 per cent. Consols, "to provide in perpetuity a yearly bonus of £50, to be given to the recipient of the Copley Medal." So long as the interest was at 3 per cent., the income of £50 was produced, but now that it is reduced to $2\frac{3}{4}$ per cent., with the near prospect of falling to $2\frac{1}{2}$ per cent., it is insufficient for the gift. So long, however, as there has been a balance in hand in favour of the fund, the Council has thought well to fulfil Sir Joseph Copley's liberal intention.

No. 5. The Keck Bequest.

In the year 1719 a bequest of £500 was received from Mr. Robert Keck, who directed that the profits arising from it were "to be bestowed on some one of the Fellows, whom they shall appoint to carry on a foreign correspondence." For many years this bequest was merged in the general funds of the Society, and the proceeds applied towards the payment of the Foreign Secretary. In 1881 it was again made to appear as a separate trust fund, and £666 13*s*. 4*d*. Consols was allotted as the equivalent. This has, at the present time, been converted into £800 Midland Railway 3 per cent. Debenture Stock, and the proceeds are annually paid to the Foreign Secretary, who now receives no other honorarium.

No. 6. The Wintringham Fund.

In 1794 a sum of £1200 Consols was bequeathed to the Society by Sir Clifton Wintringham, M.D., a Fellow, payable on the decease of his widow, and subject to certain conditions. It was not, however, until 1842 that, after a tedious law suit, an amount of £1200 Consols was transferred to the Society. It was then found that the conditions of the will were so stringent, and involved so much expense, that it was practically impossible to fulfil them, even when the rate of interest on Consols was 3 per cent. instead of, as at present, $2\frac{3}{4}$, or, as it will be shortly, $2\frac{1}{2}$ per cent. There being a further provision that in case of failure on the part of the Society to fulfil the intentions of the testator, the income of the fund should be paid over to the Governors of the Foundling Hospital, that institution has in each year received the interest accruing from the fund. The subject has on several occasions been brought before the Council, and also before the legal advisers of the Society, but as yet no way out of the difficulty has been discovered.

No. 7. THE CROONIAN LECTURE FUND.

This is one of the earliest institutions connected with the Society, and, in name at least, carries us back to the days of its foundation. At the meeting held on November 28, 1660, when the design for founding the Society was discussed, Mr. Croone, though absent, was nominated as the Register, or as we should now call it Registrar, of the small band of learned men who met weekly at Gresham College. Dr. Croone, as he subsequently became, was from the beginning an active Fellow of the Society, and on his death, in 1684, left a scheme for two lectureships which he intended to found, one of which was for the Royal Society. In his will, however, he made no provision for this purpose, but his widow, who subsequently became Lady Sadleir, remedied the omission, and in her will, dated September 25, 1701, bequeathed to the Society one-fifth of the clear rent of the King's Head Tavern, in or near Old Fish Street, London, at the corner of Lambeth Hill, "for the support of a lecture and illustrative experiment for the advancement of natural knowledge on local motion, or (conditionally) on such other subject as, in the opinion of the President for the time being, should be most useful in promoting the objects for which the Royal Society was instituted." A decree in Chancery, in 1728, empowered the Society to devote the whole nett annual profits of the legacy to the payment for a single lecture and its attendant expenses. The proper subject for the lecture is the nature or laws of muscular motion, to be accompanied by some anatomical demonstration. The first Croonian Lecture was delivered in 1738 by Dr. Stuart, the subject being "The Motion of the Heart." From 1786 to 1885 the property was let for £15 per annum, so that the share of the Society was only about £3, but since 1885 the rent of the estate has been materially increased, and the Society now receives a sum of about £52 yearly as its share, which is paid over by the Royal College of Physicians. The whole of the available balance is in each year paid to the lecturer or for expenses, but the account at the present time is slightly overdrawn on account of the expenses of the last lecture having been unusually high.

No. 8. THE DAVY MEDAL FUND.

By the will of Dr. John Davy, F.R.S., the service of plate presented to Sir Humphry Davy for the invention of the safety lamp, was bequeathed to the Society, to be melted down and sold, in order to found a medal to be given annually for the most important discovery in chemistry. The amount received in 1869 was invested in the purchase of £660 Madras Railway Stock, producing about £33 per annum. Some little time elapsed before the dies could be prepared, and the first medal actually awarded was given, in duplicate, to Bunsen and Kirchhoff in 1877. It was also given in duplicate in 1878, 1882, 1883, and 1893, but the fund has still a balance in hand of £48 17*s*. 9*d*.

No. 9. THE GASSIOT TRUST.

In the year 1871 the late Mr. John Peter Gassiot conveyed to the Society £10,000 Italian Irrigation Bonds, for the purpose of assisting in carrying on and continuing magnetical and meteorological observations with self-recording instruments, and any other physical investigations that may from time to time be practicable and desirable in the Kew Observatory, in the Old Deer Park, Richmond, Surrey.

The proceeds are paid over to the Kew Committee appointed in accordance with the trust deed. From time to time some of the Irrigation Bonds are drawn, and a profit has been made on reinvestment. These accumulated profits are now represented by a sum of £400 2¾ per cent. Consols, which forms a kind of reserve or insurance fund. The balance in hand in November last was £164 10*s*. 7*d*.

No. 10. THE HANDLEY FUND.

By the will of Mr. E. H. Handley, dated 1840, the reversion of his property was bequeathed to the Society after the death of his sister, the income to be applied as a reward for important inventions in art or discoveries in science, physical and metaphysical, or for assistance in the prosecution of any such invention or discovery, but with power to the President and Council to apply the income as they may deem best for the advancement of science.

Owing to the Statute of Mortmain, a considerable portion of the property did not pass by this will, but eventually, in 1876, the sum of £6378 19*s*. was received, which, after paying Legacy Duty at the rate of 10 per cent. and legal expenses, left sufficient to purchase £6047 7*s.* 9*d.* Reduced 3 per cents. When the rate of interest was threatened a few years ago, this was converted into £4798 Lancashire and Yorkshire Railway 4 per cent. Guaranteed Stock, producing about £195 per annum. Of late years this has been applied towards the cost of preparing the Catalogue of Scientific Papers.

No. 11. THE JODRELL FUND.

The late Mr. T. J. Phillips Jodrell, in 1876, placed at the disposal of the Society the sum of £6000, at first with the intention of encouraging in this country original research in the physical sciences, but subsequently, in the same year, with directions to apply the proceeds as part of the ordinary revenue of the Society. In 1879 £1000 was, by Mr. Jodrell's directions, transferred to the Fee Reduction Fund, and the remaining £5000 is represented by the sum of £5182 14*s.* 10*d.* 2¾ per cent. Consols, which stand in the name of the fund.

On the death of Mr. Jodrell, in 1889, the proceeds of the fund, in accordance with a letter from him of April 5, 1878, devolved to and were incorporated with the Donation Fund. The income is at present about £140, but this will be reduced when the diminution in the interest of Consols takes place.

No. 12. FEE REDUCTION FUND.

This fund originated in 1878, the object being to relieve future Fellows of the Society of the £10 paid as an admission fee and of £1 out of the £4 annual subscription. These advantages, however, do not extend to the Privy Councillors and other privileged Fellows who join the Society. Most liberal sums were subscribed: Sir Joseph Whitworth contributing £2000, Sir William (now Lord) Armstrong and Mr. James Young £1000 each. The demand upon the fund keeps on, of course, increasing from year to year, but the excess of income over expenditure has been regularly invested, and the fund now consists of £4900 Metropolitan 3½ per cent. Stock and £9333 London and North-Western Railway 3 per cent. Debenture Stock, producing an income of about £450 per annum. The payments on account of Fellows amounted last year to £342, and inasmuch as in each year the payment in respect of the subscriptions of Fellows increases to the extent of £10 or £12, it is evident that, in the course of time, the question will have to be considered whether some modification in the amount of the reduction or some addition to the capital of the fund must not be made. The payments last year were in respect of the contributions of 192 Fellows and the admission fees of 15.

There is, however, at present over £100 per annum left for investment; so that there is no immediate danger of the fund failing. Meanwhile, if any Fellow wishes to relieve the fund of the annual payment of £1 on his account, the Treasurer will be glad to hear from him.

No. 13. THE DARWIN MEMORIAL FUND.

In 1885 the Committee of the International Darwin Memorial Fund resolved to transfer to the Royal Society the balance that remained in their hands, in trust, to devote the proceeds from time to time towards the promotion of biological studies and research. The amount was invested in the purchase of £2200 South-Eastern Railway 4 per cent. Debenture Stock, which now forms the capital of the fund. The annual income is about £88. In accordance with a resolution of the Council, a medal, either in silver or in bronze, is awarded biennially in reward of work of acknowledged distinction (especially in Biology) in the field in which Mr. Darwin himself laboured. The medal is accompanied by a grant of £100, and the balance of the proceeds is from time to time to be added to the capital fund. At the November audit the balance was £461 18*s.* 10*d.*, but out of this the grant of £100 to the recipient of the medal was paid at the anniversary.

No. 14. THE JOULE MEMORIAL FUND.

In 1890 the Joule Memorial Committee transferred to the Society the balance in their hands, the proceeds to be applied for the encouragement of research, both in England and abroad, especially amongst younger men, in those branches of physical science more immediately connected with Joule's work. According to the regulations made by the Council, a studentship or grant is to be made every second year to assist research as already specified. These grants are to be made alternately in Great Britain and abroad. The fund consists of £1000 London Brighton and South Coast Railway Guaranteed 5 per cent. Stock and £50 on deposit, the annual income being about £50. The first scholarship of the value of £100 was conferred on Mr. J. D. Chorlton, of Owens College, Manchester, in June, 1894, but when the accounts were made up no payment had been made to him, and the balance in hand appeared as £79 19*s.* 4*d.* The half-year's payment has since been made.

No. 15. THE BRADY LIBRARY FUND.

The late Mr. Henry Bowman Brady bequeathed to the Society in 1891 all his books and papers relating to the Protozoa, and also a sum of £300, the interest of which, or the principal, or both, are from time to time to be applied in the purchase of works on the same or kindred subjects to be added to the collection. The fund, which has been placed on deposit at the bankers, now amounts to £311 19*s*. It is proposed to invest a portion of it in Consols.

No. 16. THE GUNNING FUND.

In 1891 His Excellency Dr. Robert Halliday Gunning gave the Society his bond for £1000 bearing interest at 4 per cent. to form a fund the annual income of which shall be applied triennially towards the promotion of Physical Science and Biology in such manner as to the President and Council may appear most desirable. The three years' income, amounting to £120, has now been received, and the disposal of it awaits the decision of the President and Council.

No. 17. THE BUCHANAN MEDAL FUND.

This fund dates from February, 1894, when a sum of £276 12*s*. and the dies for a medal were offered to the Society by the Committee of the Buchanan Fund. The amount has been invested in the purchase of £258 9*s*. 2*d*. Metropolitan 3 per cent. Stock, producing rather less than £8 per annum. The medal, which is to be of gold and of the value of about twenty guineas, is to be awarded every three or five years for distinguished service in Hygienic Science or Practice, in the direction either of original research or of professional, administrative, or constructive work. The balance in hand is to accompany the medal, which is to have no limit as to nationality. The first medal was given to Lady Buchanan by the subscribers to the fund.

Such is a brief account of the seventeen Trusts which at the present time the Society is called upon to administer, and I have only to add that the whole of the accounts of the Society, whether for general purposes or for the Trust Funds, are under the immediate care of Mr. W. B. Keen, a chartered accountant of the highest standing.

II. "The Latent Heat of Evaporation of Water." By E. H. GRIFFITHS, M.A., Sidney Sussex College, Cambridge. Communicated by R. T. GLAZEBROOK, F.R.S. Received December 28, 1894.

(Abstract.)

Section I.

Although the enquiry described in the paper,* of which this communication is an abstract, has engaged my attention for the last two years, the actual experiments on which the conclusions are based were not performed until the months of September and October, 1894. Many difficulties in the construction of the apparatus had to be overcome, also the necessary standardisation of the instruments occupied my leisure time for some months.†

The apparatus was designed so as to enable me to perform experiments at temperatures from 10° to 60° C., and I hoped to carry out my investigations over that range. Owing, however, to incidents described in Paper L, I have been able to complete the enquiry at two temperatures only, although a number of less satisfactory experiments, performed in a different manner from that finally adopted, give approximate determinations at other temperatures.

The apparatus is, I regret to say, of a very complicated nature, owing, in a great measure, to my desire to avoid observational errors by as far as possible so arranging the machinery as to make it self-recording and self-regulating. I do not, therefore, feel it possible to convey any clear idea of it in this abstract, and will confine myself to a short description of the method, and the communication of the conclusions to which the experiments have led me.

The calorimeter was suspended by glass tubes in a chamber, the walls of which were maintained at a constant temperature (θ_0). The methods by which θ_0 was kept constant have been fully described in a previous paper.‡ Since the publication of that communication some additions have been made by which the apparatus has been so improved that it may be regarded as almost perfect in its action over the range 10° to 60°.

A silver flask which contained the water to be evaporated was placed within the calorimeter, and the whole of the calorimeter exterior to the flask was filled originally with aniline, and afterwards with a singularly limpid oil, consisting of hydrocarbons only. This oil was stirred by paddles revolving about 320 times per minute. Within the oil was a platinum-silver coil whose ends were kept at a known potential difference when an experiment was proceeding. The supply of heat was balanced by the loss due to evaporation in the flask, and thus the temperature of the calorimeter (θ_1) was kept practically constant and equal to θ_0 throughout an experiment. True θ_1 oscillated slightly about θ_0, but $\theta_1 - \theta_0$ rarely attained to 0·05° C. in experiments lasting 70 or 80 minutes, and the oscillations could be so controlled that $\theta_1 - \theta_0$ was alternately positive and negative. The advantages of the above method are that the results are not appreciably affected by—

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* Referred to in this abstract as "Paper L."

† Throughout this enquiry I have been assisted by Mr. C. Green, Scholar of Sidney Sussex College, Cambridge, to whom I return my sincere thanks.

‡ "The Mechanical Equivalent." 'Phil. Trans.,' 184 (1893), A, pp. 361—504.

(1.) Errors in thermometry.
(2.) Changes in the specific heat of water.
(3.) The capacity for heat of the calorimeter.
(4.) Loss or gain by convection, &c.

Had the calorimeter been filled with water, its capacity for heat would have been so great that a considerable loss or gain of heat might have caused but small change in θ_1, hence the employment of the oil, above referred to, whose specific heat and specific gravity were both small, and thus its "volume heat" was only about four-tenths that of water. It was necessary to detect, and in some cases to measure, very small differences between θ_0 and θ_1, and this was done by the use of differential platinum thermometers. A full description of these thermometers has recently been published.* Extremely minute differences of temperature could not only be detected, but also be measured by means of the arrangement adopted. In Paper L I describe the experimental proofs of the following statement: "It follows that the differences of temperature could be determined to 0·0004° C., and I am confident that differences of 0·0001° C. could be detected."

Had it been possible to secure (*a*) that θ_0 should remain absolutely unchanged; (*b*) that the value of $\theta_1 - \theta_0$ should be zero at the commencement and also at the end of an experiment, it would have been unnecessary to *measure* differences of temperature, since the variations during an experiment would, if the above conditions were observed, have no effect on the result. Let θ_1' and θ_1'' be the initial and final temperatures of the calorimeter, and C_{θ_1} the capacity for heat of the calorimeter and contents at the temperature θ_1, then $C_{\theta_1}(\theta_1' - \theta_1'')$ gives the loss of heat of the calorimeter, and as I have shown that the loss or gain by radiation, &c., may be neglected, it follows that this heat was abstracted by the evaporation of some water. Also, if θ_0 varied, an additional correction had to be made. I have indicated by the expression Σq the supply of heat during an experiment due to the above sources.

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* "The Specific Heat of Aniline." 'Phil. Mag.,' January 1895.

A source of heat was the supply due to the stirring. Let thermal units per second due to this cause be Q_s, and let the stirring be maintained for a time t_s, then total mechanical supply $= Q_s t_s$. I have given full particulars of the manner in which the values of Q_s for different values of θ_1 were ascertained (Q_s increased as θ_1 diminished, owing to the increase in viscosity of the oil). A study of the results of the stirring experiments led to the following conclusion: "The values of Q_s given in the tables are certainly correct to better than 1 in 50 at temperatures 30° and 40°, for in no case do individual experiments (when reduced to the same rate) differ by 1 in 100. Now an error of 1 in 50 in Q_s would cause an error of only about 1 in 5000 in L. I am less certain, however, about the values of Q_s at 20° and 50° C."

The last, and by far the greatest source of heat, is the work done by the electric current. Let Q_e be the units per second due to this cause, and let the current be maintained for a time t_e.

Now $$Q_e = \frac{e^2 n^2}{R_1 J} \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1),$$

where e is the potential difference of a Clark cell at 15° C., n the number of cells, and R_1 the resistance of the coil at temperature θ_1, after the corrections for the rise in temperature of the wire due to the passage of the current, &c., have been made.*

Let m be the mass *in vacuo* of water evaporated.

Then $$ML = Q_e t_e + Q_s t_s + \Sigma q \ldots\ldots\ldots\ldots\ldots\ldots (2).$$

In order to give an idea of the comparative importance of the terms in equation (2), I give the *mean value* of each term resulting from the experiments.

Table I.

	$Q_e \times t_e$.	$Q_s \times t_s$.	Σq.
When $\theta_1 = 40$ and $n = 4$	2150	19·2	±1·6
$\theta_1 = 30$ and $n = 4$	2305	32·9	±1·2
$\theta_1 = 30$ and $n = 3$	1752	32·9	±1·2

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* The value of J used in these reductions was $4·199 \times 10^7$. If there is some error in the electrical standards, &c., by which I determined J, this is still the right value to use here, for the standards are the same, and the methods practically

There can be no question as to the accuracy with which $Q_e t_e$ was measured. The term $Q_s t_s$ is probably the one least accurately determined, but the probable error is not 1 in 100, and the quantity Σq was found to a far greater degree of accuracy than the above table shows to be necessary, as it was certainly known to 1 in 1000.

I will not here attempt any proof of the above statements, full particulars being given elsewhere, nor do I propose to describe the mode of carrying out an experiment, as I find it impossible to sufficiently compress the account of the operations.

I performed a large number of experiments when the evaporation of the water was promoted by the passage of a gas. Extreme precautions were taken to dry the gas and to prevent any carrying of heat by it either to or from the calorimeter, but the results of these experiments cannot be called satisfactory, as the differences between individual experiments at the same temperature in extreme cases amounted to as much as 1 per cent. The following table gives the results:—

Table II.

No. of expts.	Mean Temp.	L.
5	49·82	566·5
10	39·99	572·4
9	24·96	581·9

The rate of evaporation was greatly varied during these experiments, for the potential difference was changed from that of 1 to 3 Clark cells, hence the rate of evaporation was in some cases nine times as great as in others.

There is one curious coincidence, which, however, may be merely fortuitous. In two of these experiments nitrogen was passed through the water instead of air, and these two experiments give almost exactly the same value as my final results, viz.:—

Temp.	L (Nitrogen expts.).	L. Final values
24·96°	581·68	581·7
39·98°	572·72	572·7

The method finally adopted was that of allowing the water to fall drop by drop on to the interior surface of the silver flask, and causing

identical, and it will be seen that any errors of this kind would be eliminated during the calculations. This remark, however, does not apply to Professor Schuster's correction for the specific heat of the air displaced, for we are not dealing here with differences in rate of rise caused by the displacement of air by water. His correction (1 in 4000) is therefore included in the above value.

it to boil off rapidly by sufficiently diminishing the pressure. The object of this arrangement was to ensure, as far as possible, that the temperature of the evaporating water was that of the flask (θ_1). Many precautions too numerous to be here referred to had to be taken before this method could be worked successfully, but when once the experimental difficulties had been overcome, the agreement between different experiments left little to be desired, and this continued to be the case although the conditions of the experiments were considerably altered. Different masses of water and different rates of evaporation gave closely concordant values of L.

Table of Results.

(An improvement was made in the apparatus after No. 5, and I attach greater weight to all succeeding experiments.)

Table III.

No. of expt.	Temp.	L.	No. of expt.	Temp.	L.
I	40·147°	573·11	XII	29·987	578·58
II	40·146	572·31	XIII	29·983	578·64
III	40·147	572·77	XIV	29·998	578·78
IV	40·144	572·61	XV	29·999	578·90
V	40·145	573·80	XVI	30·004	578·60
			XVII	29·993	578·83
VI	40·147	573·01	XVIII	29·993	578·60
VII	40·147	572·50			
VIII	40·147	573·00	Means	29·994	578·70
IX	40·149	572·28			
X	40·157	572·12			
XI	40·133	572·61			
Mean of all	40·146	572·74			
Mean of VI to XI	40·147	572·59			

I regret that the experiments are comparatively few in number, but they occupied so much time that I was unable to make further repetitions.

I do not, however, consider that such repetitions would have greatly strengthened the evidence, for the mean probable error of even a small group of experimental results like the above is less than the probable error of some of the constants involved in the reduction of those results.

For the reason previously stated I attach small value to Nos. I to V.

My conclusion therefore is—

Temp.	L.
40·15°	572·60
30·00	578·70

Section II.

On p. 218 will be found, I think, a fairly complete table of results published since the year 1843.

The values obtained by Winkelmann ('Wied. Ann.,' 9, 1880) are not included in the table, as they are not based on independent experiments, but deduced from the observations of Regnault.

In Paper L I have given reasons for my conclusion that much greater weight attaches to the values of Dieterici and Regnault than to those given by the other observers.

Dieterici's method was to determine the mass of ice formed by the evaporation of a certain mass of water at 0°. Hence, his results, like my own, are independent of thermometric errors, or errors in the determination of the water equivalent of his apparatus. Certain of his experiments were performed by placing the water to be evaporated in platinum, instead of glass, tubes, and to these he attaches, I think rightly, greater importance than he does to the remaining experiments. His conclusions are as follows:—

"Die Versuche mit dem Platingefässe ergeben

$$L = 596·73,$$

mit einem wahrscheinlichen Fehler des Mittels von $\pm 0°·13$."*

He succeeded in altering the rate of evaporation very greatly, and that without affecting his results. I cannot here even enumerate other considerations which lead to the conclusion that the result of Dieterici's platinum tube experiments carries great weight.

As stated in the above table, Regnault performed twenty-two experiments over the temperature range −2° to +16°. There can be little doubt, however, that his results over this range are of small value, and he was evidently of this opinion himself.†

Winkelmann has written a criticism of Regnault's experiments,‡ in which he comes to the conclusion that it is necessary to reject this series of experiments, and, in Paper L, I give additional reasons for their rejection. The chief arguments are as follows:—

(1.) Regnault determined the temperature of the vapour in the spiral by observations of the *pressure* in the condenser.

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* 'Wied. Ann.,' vol. 37, 1889, p. 504.

† See 'Mémoires de l'Acad.,' vol. 21, 1847, pp. 712—719.

‡ 'Wied. Ann.,' vol. 9, 1880.

Table IV.

Temperature.	Observer.	No. of expts.	Extreme values for L.	Mean L.	Reference.
0°	Dieterici	20	595·52—598·84	596·8	'Wied. Ann.,' vol. 37, 1889.
−2 to +16	Regnault	22	—	—	'Mémoir. de l'Acad.,' vol. 21, 1847.
63 to 88	"	23	—	—	" " "
99·88	"	44	533·3 —538·4	536·67	" " "
99·81	Favre and Silbermann	3	532·59—541·77	535·77	'Ann. de Chimie,' vol. 37, 1853.
100	Andrews	8	530·8 —543·4	535·9	'Chem. Soc. Journ.,' 1849.
100	Berthelot	3	535·2 —537·2	536·2	'Comptes Rendus,' vol. 85, 1877.
100	Schall	—	No details given	532·0	'Ber. d. Chem. Ges., vol. 17, 1884.
100·16	Hartog and Harker*	5	523·61—525·87	524·60	'Manchester Phil. Soc. Proc.,' 1893-94
119—194	Regnault	73	—	—	'Mémoir. de l'Acad.,' vol. 21, 1847.

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* It is right to add that Messrs. Hartog and Harker state that these are the results of "Preliminary Experiments," and should not be regarded as giving their final conclusions. I do not, therefore, include their work in my criticisms.

This must have been (as Regnault himself points out) considerably below the pressure of the vapour in the flask.

(2.) At low temperatures a small error in the pressure would cause a considerable error in temperature. For example, an error of 0·4 mm. in pressure would correspond (at 4° C.) to an error of 1° in θ.

(3.) All his observations of the change in temperature of the calorimeter at low temperatures were taken on a falling mercury thermometer, and an error of 0°·01 (the limit to which he observed his thermometer) would change the resulting value of L by 1 in 500. Now, no observations on falling mercury thermometers can be relied on to give an accuracy of anything like the above order.

(4.) The extreme divergence between the results of these experiments would alone render them of small value.

(5.) Regnault's formula for the "total heat" ($606·5+0·305\,\theta$) is not in agreement with his experimental results.

I have in my paper given further reasons for their rejection, but I think the above are alone sufficient to prove that at any rate the results of these experiments must be regarded with suspicion. I have also shown that the tendency of all the above sources of error is to make Regnault's values of L too high at these temperatures.

These above objections, however, lose all their force when applied to Regnault's determinations from 63° to 100°. His methods of experiment were entirely altered; for example, p was observed where the vapour was formed, and a large change in p would produce but a small change in θ. Also his thermometers were rising, the range of temperature was more than twice as great, and his results were far more uniform, &c.

At about 100° Regnault performed forty-four experiments, of which he rejects the first six as "preliminary"; the remaining thirty-eight give the following mean results:—

Temp.	"Total heat."
99·88	636·67

This would become 636·60 at 100°. Hence, if we assume that 1 gram of water in cooling from 100° to 0° gives out 100 thermal units, we get L = 536·60 at 100°.

Now, taking my values, viz.,

Temp.	L.
40·15°	572·60
30·00	578·70

We get $$d\mathrm{L}/d\theta = 0·6010.$$

If we assume with Regnault that L is a linear function of θ we can deduce the values at 0° and 100°, and we get 596·73 and 536·63 respectively.

Hence

Table V.

	Values of L.	
	0°.	100°.
Regnault	—	536·60
Dieterici	596·73	—
Griffiths (extrapolated)	596·73	536·63

I think that I am justified in calling this agreement remarkable, and there is evidence that it is not a mere coincidence.

Winkelmann, in his analysis of Regnault's work, states that the following formula gives the results of Regnault's experiments with greater accuracy than Regnault's own formula:—

$$L = 589{\cdot}5 - 0{\cdot}2972\,\theta - 0{\cdot}0032147\,\theta^2 + 0{\cdot}000008147\,\theta^3 \;\ldots.\; (W).$$

Now if we assume my value of $dL/d\theta$ we get

$$\text{“Total heat”} = 596{\cdot}73 + 0{\cdot}3990\,\theta \;\ldots\ldots\ldots\ldots\; (G_2),$$

whereas Regnault's formula is,

$$\text{Total heat} = 606{\cdot}5 + 0{\cdot}305\,\theta \;\ldots\ldots\ldots\ldots\ldots\ldots\; (R).$$

The following table gives in column III all Regnault's experimental results (R_e) below 100°, except those (below 63°) by his other mode of experiment, which I have given reasons for rejecting. Column VII shows the difference between R_e and the value given by formula (R). Column VIII gives $R_e-(W)$, and column IX $R_e-(G_2)$.

Table VI.

Comparison between Regnault's Experimental Results (R_ϵ) over the range 63° to 88°, with the value given by formulæ (R), (W), and (G_2).

I. No. of expt.	II. Temp.	III. Experimental results (R_ϵ).	VII. $R_\epsilon - R$.	VIII. $R_\epsilon - W$.	IX. $R_\epsilon - G_2$.
1	88·11°	633·4	0	+0·9	+1·5
2	87·83	633·1	−0·1	+0·8	+1·3
3	85·97	628·4	−4·3	−3·3	−2·6
4	85·24	628·6	−3·9	−2·9	−2·1
5	85·20	631·7	−0·8	+0·2	+1·0
6	84·68	629·9	−2·4	−1·3	−0·6
7	83·08	628·9	−2·9	−1·8	−1·0
8	82·66	631·0	−0·7	+0·4	+1·3
9	81·03	628·8	−2·4	−1·2	−0·3
10	80·60	627·7	−3·4	−2·2	−1·2
11	80·37	628·8	−2·2	−1·0	0·0
12	80·17	630·2	−0·7	+0·5	+1·5
13	79·55	630·1	−0·7	+0·7	+1·6
14	78·28	627·0	−3·3	−2·0	−1·0
15	76·50	628·6	−1·2	+0·3	+1·4
16	71·35	624·4	−3·8	−2·0	−0·8
17	71·11	622·2	−5·9	−4·1	−2·9
18	70·49	626·9	−1·1	+0·8	+2·0
19	69·70	626·4	−1·3	+0·7	+1·9
20	68·01	622·5	−4·8	−2·6	−1·4
21	66·30	624·7	−2·0	+0·2	+1·5
22	64·34	622·9	−3·2	−1·0	+0·5
23	63·02	625·5	−0·2	+2·3	+3·6
Sum of differences			−49·3	−17·6	+5·2
Mean difference			−2·14	−0·77	+0·23
If we omit Expt. 23 we get Sum of differences			−49·1	−19·9	+1·6
Mean difference			−2·23	−0·90	+0·07

We thus see that formula (G) gives a closer approximation to Regnault's experimental results over the above range than either of the other formulæ.

Again, the results of my earlier experiments, referred to above, lend support to formula (G).

One difficulty, however, presents itself. If there is any truth in the ordinarily accepted ratio of the "mean thermal unit from 100° to 0°" to the "thermal unit at 15° C.," then, so far from my own, Dieterici's, and Regnault's results being in agreement, they in reality differ considerably, for our values are expressed in different units. True, Regnault adopted the "thermal unit at 15°," but, as his experi-

Table VII.

Temp.	L from pressure expts. (*supra*).	L. Formula (G).
25·0°	581·9	581·7
40·0	572·4	572·7
49·8	566·5	566·8

ments gave the "total heat," not the value of L, the results should be different after subtraction of the "total heat" of the water, for when subtracting we assumed the identity of the two units.

Dieterici adopted as the basis of his measurements the mass of mercury drawn into a Bunsen's calorimeter by the heat evolved from 1 gram of H_2O when cooling from 100° to 0°.* Now, according to Regnault,† the ratio of the "mean thermal unit" to the "thermal unit at 0° C." is as 1·005 : 1.

If we assume Rowland's, or Bartoli and Stracciati's, determination of the changes below 15° (my own have not extended below that temperature), we should get

$$\frac{\text{mean thermal unit}}{\text{thermal unit at }15^\circ} = \frac{1{\cdot}005}{0{\cdot}994}\text{ approximately} = \frac{101{\cdot}1}{1},$$

and thus Dieterici's value of L at 0°, if expressed in terms of thermal unit at 15°, would become 603·3.

Again, according to Regnault we ought to subtract 100·5 from 636·60, and thus get L at 100° = 536·1.

The only reasonable explanation of this difficulty appears to me to be that the value of the "mean thermal unit" is practically the same as the value of the "thermal unit at 15° C."

I see nothing impossible in this supposition. As shown in Paper L, there is sufficient evidence that at low temperatures the capacity for heat of water decreases with rise of temperature. Rowland found a minimum indicated near 34°. If, therefore, the capacity for heat increases gradually above some such temperature, but more rapidly near 100°, it is quite conceivable that the "mean thermal unit" should closely approximate to the "thermal unit at 15° C." Our only experimental evidence to the contrary is that given by Regnault in his paper "De la Chaleur Spécifique." We know that his conclusions at low temperatures are incorrect, and I do not see that those at higher temperatures have greater value, for his methods of observation and experiment were in this case unaltered. The matter, of course, can only be cleared up by a direct determination of the capacity for heat of water over the range 0° to 100°.

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* 'Wied. Ann.,' vol. 37, 1889, p. 499.

† "De la Chaleur Spécifique." 'Acad. des Sciences,' tome xxi.

In Paper L, I give further evidence in support of the equality of the two units.

Considering the extreme attention which has been given during the last few years to the determination of the electrical units, it is strange that so little has been done regarding what is perhaps as important a unit, viz., that of heat. I venture to appeal to the Royal Society to take steps to place our knowledge in this respect on a firmer basis, and I would go so far as to express my belief that the method of measuring small differences of temperature described in Paper L, and also in the communication printed in the 'Philosophical Magazine' of this month, points out a way to the solution of some of the difficulties.

Section III.

The density of water-vapour at different pressures can be obtained from the thermodynamic equation $L = \frac{T}{J}(s'-s)\frac{dp}{dT}$. In Paper L, I have given the density at different pressures thus obtained by the substitution of my values of L and J.

We can also obtain what Winkelmann terms the "theoretical density" by assuming that water-vapour behaves as a perfect gas, having the same molecular weight. I have shown that if we take the most recent determinations of the atomic weight of oxygen,* the "theoretical density" of water-vapour at low pressures is almost identical with the density as deduced from the thermodynamic equation. At higher pressures (above 140 mm.) the density appears to remain nearly constant, and is about 1·02 times as great as the "theoretical density."

These conclusions are confirmed by a study of the "volume energy" of water-vapour at different temperatures.

Conclusion.

The results obtained by Dieterici at 0° C., by Regnault at temperatures 63° to 100° C., and by myself at intermediate temperatures, are represented with great accuracy by the formula

$$L = 596{\cdot}73 - 0{\cdot}6010\,\theta.$$

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* Scott, 'Phil. Trans.,' A, 1893, p. 507.

III. "On Slow Changes in the Magnetic Permeability of Iron." By WILLIAM M. MORDEY. Communicated by Professor SILVANUS P. THOMPSON, F.R.S. Received December 19, 1894.

When iron is magnetised for a long time by rapidly alternating currents, its magnetic permeability is usually reduced, a gradual increase taking place in the amount of energy absorbed in producing a given magnetisation. This effect has been observed in connection with the working of transformers on alternate-current systems. Although it has been known for some time to a few electrical engineers, the author is not aware that any investigation or proof has been published as to the cause of this change.*

When the author first became aware of this increase he investigated the subject in connection with the work of the Brush Electrical Engineering Company, and to that company acknowledgments are due for permission to publish the results so far obtained. It may be mentioned that the investigation is still in progress.

In the first place explanations were sought in direct connection with the magnetic or electric actions that take place.

The explanation that first suggested itself was that eddy currents were being set up in the coils by leakage or partial failure of the insulation between adjacent portions of the conductor, or that eddy currents were being set up to an increased degree in the iron by disturbance or change of the insulating material interposed between the thin plates of iron of which the transformers were composed.

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* The following are the only published references to this subject, so far as known to the author:—

In the 'Electrician' of December 7th, 1894, the subject is introduced by Mr. G. W. Partridge, with some examples of the increase and a statement that the effect is due to a "molecular change or fatigue in the iron."

There is also a note by Professor Ewing referring to Mr. Partridge's article, also ascribing the effect, if confirmed, to "a progressive magnetic fatigue."

In the same journal for December 14th, 1894, there is an article on the subject by Dr. Fleming, stating that the effect is not general, that he has failed to find it in some cases, and pointing out that differences of temperature at the times of testing may account for the variations observed.

Mr. R. T. Smith states that in one sample of iron he has found considerable increase after about 240,000,000 reversals.

Mr. O. T. Blathy, of Buda-Pesth, states that he is familiar with the effect, that the loss in magnetising transformers increases from 20 to 25 per cent., and that artificial heating of the transformer for several hours to about 150° C. will have the same effect, which he states is permanent.

Sir David Salomons also states that he has referred to the effect in a work written some months ago.

A careful examination showed that this explanation was untenable. The increase was found in transformers, the coils of which were thoroughly dry and well insulated, and in which no change had occurred in the separating material between the plates. This was confirmed by removing the iron and trying it with new coils specially insulated—also by trying the old coils with other samples of iron which had been separately tested.

Another explanation suggested was that the effect was a sort of magnetic fatigue—that the iron deteriorated somewhat in the same way as with steel springs, which suffer from "fatigue" after a great number of extensions or vibrations.

The gradual increase in the energy absorbed pointed to a physical change in the iron, which behaved exactly as if it slowly hardened and so became less permeable—as if the softening results of the process of annealing were being gradually lost.

Effect of Annealing.—On carefully re-annealing some of the iron by heating it to redness and cooling slowly, it entirely recovered its original high permeability.

Having thus found that the iron was not permanently or irremediably affected—although apart from annealing the effect seems to be permanent—it remained to ascertain whether the change was due directly to the magnetic action, or whether it was caused by some secondary condition, having the reversals of magnetism as a primary cause.

Effect of Heat.—An investigation was therefore made as to the effect of heat on the iron.

Under the conditions of working, the temperature of the iron in transformers is raised, both by the energy dissipated in the iron itself by hysteresis and eddy currents, and also by conduction and radiation from the copper windings forming the primary and secondary conductors. This rise of temperature varies according to the conditions of working and the construction of the transformer; usually it is from 20° to 60° C. above the surrounding atmosphere.

In order to investigate the action of heat, as distinct from magnetic or electric action, some samples of iron were kept for some months in an oven maintained at temperatures varying from about 60° C. to about 75° C. These samples were of iron obtained for transformer construction and were built up in the manner of transformers, having windings which were used simply for testing purposes.

Method of Measuring.—The coils used were of known turns and resistance. The wire was of such a size as to make the C^2R loss negligible in comparison with the losses in the iron. The power was measured by a very sensitive wattmeter, constructed by Dr. Fleming, the volts by a Cardew voltmeter checked against a Kelvin multicellular electrostatic voltmeter, and the current by a

Siemens electro-dynamometer. These were instruments in regular use for manufacturing purposes and used in fixed positions. The readings of watts and volts are reliable within about 0·5 per cent. The current readings are less accurate, as the range of the instrument was not very suitable. The source of current was a 37-kilowatt alternator of the author's type, working at 100 periods per second, and at a high E.M.F. The low E.M.F. required for the tests, from 20 to 60 volts, was given by the secondary of a transformer, the primary of which was connected to the alternator. Variations of E.M.F. were obtained by adjustment of the field excitation of the alternator.

There are irregularities in the tabulated readings which are probably not entirely due to errors of observation, but to differences of the conditions under which the tests were made. For example, the iron when removed from the oven was allowed to cool, but the exact temperature was not taken, and it certainly was not the same in all cases. In August it would probably be higher than in December. And although the alternator used to supply the testing current was the same in all cases and was run at the same periodicity, there were differences of condition of the circuit and load which may have had a slight influence on the results.

Sample No. 1. This consisted of a block built up of stampings arranged as in Fig. 1, which shows section and plan, the outside dimensions of the plates being 16½ ins. by 11 ins. The sheets were 0·014 in. (0·354 mm.) thick, varying slightly; they were built up to a certain thickness with 100 stampings separated by paper, the total weight being about 55 lbs. The winding is shown at C.

Samples Nos. 2 and 3 were made up exactly the same as No. 1, but were taken from different supplies of iron.

They were occasionally removed from the oven, allowed to cool, and then returned to the oven after a test had been taken of the loss of energy with a given magnetisation.

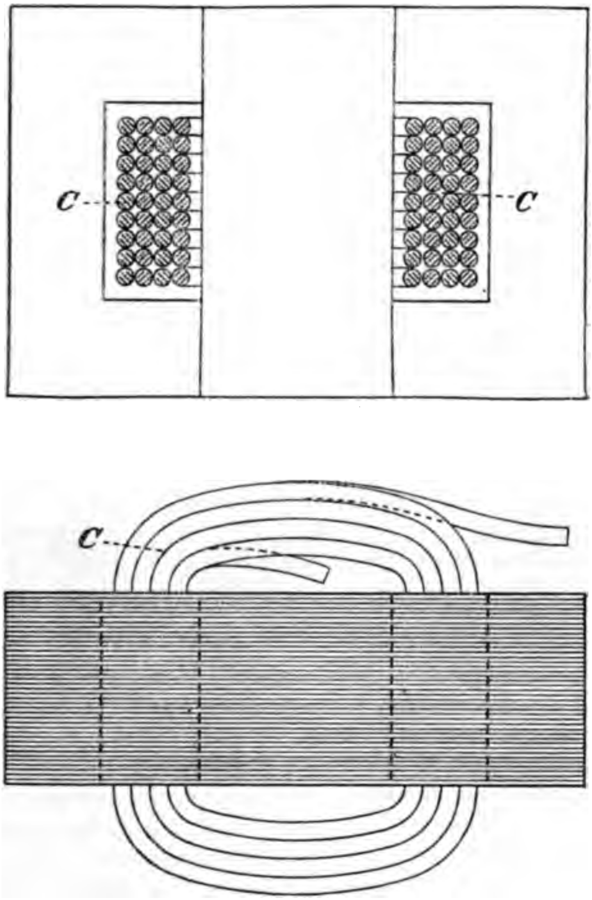
The results obtained from these three samples are given respectively in Tables 1, 2, and 3, and graphically in Diagrams 1, 2, and 3. For ease of comparison the watts are plotted as percentages in all the diagrams.

The magnetising coil in each of these cases consisted of 100 turns of copper strip 0·16 in. by 0·09 in., having a resistance of 0·15 ohm, or so low as to make the C^2R loss negligible, as compared with the loss in the iron. For example: with the maximum current used, 0·7 ampère, $C^2R = 0{\cdot}0735$ watt, the total loss being 29·3 watts.

In the foregoing examples the iron magnetic circuit is not quite continuous: it is interrupted in part by butt joints and in part by lap joints.

Sample No. 5. In order to eliminate any errors that might con-

FIG. 1.

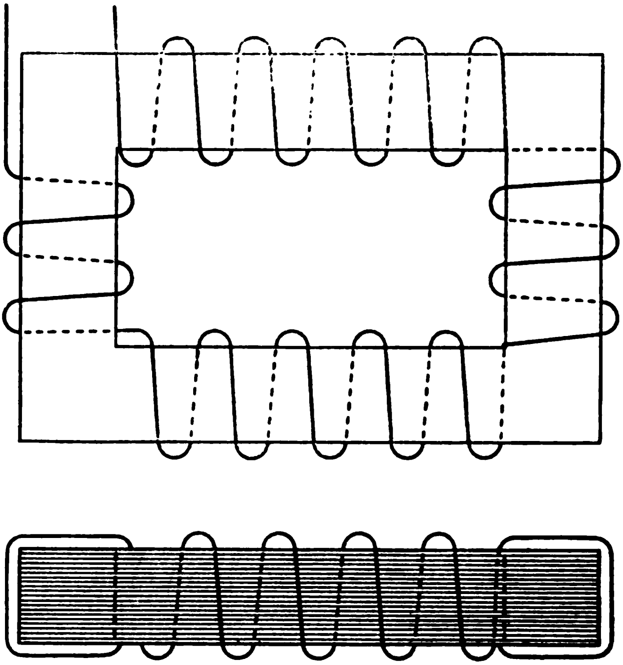


ceivably be caused by alterations at the joints, a rectangle was made up as follows:—

A number of iron plates were taken, each 16½ ins. by 11 ins. by 0·014 in., there being stamped out from the middle of each plate a piece 11 ins. by 5½ ins., as in Fig. 2, showing side and edge views. The plates were separated by paper and bound together by tape. They were then overwound, as in the figure, with a continuous winding, consisting of 140 turns of insulated copper wire 0·049 in. diameter (resistance 0·4 ohm) so disposed as to magnetise the block almost evenly throughout the magnetic circuit.

The measurements made with this sample are given in Table V and Diagram 5.

FIG. 2.



On the occasion of each test, measurements were made at three densities, viz., **B** 2500, 2752, 3050.

The sample was kept in the oven between each set of tests, at the same temperature as the others, that is, an average of about 67° C.

It will be seen that the hysteresis is still rising, the change being about in the same order as in the other samples, showing that the question of joints has no noticeable effect. The change also follows closely the same order, whether measured at 2500, 2762, or 3050 **B**.

Effect of Pressure.—An attempt was made to find if the increase was due directly to heat or to a mechanical condition brought about by the heat.

A possible explanation of the increase of hysteresis suggested itself in connection with the method of construction. In transformers the iron laminæ are usually bolted together very firmly, in order to ensure good contacts at the magnetic joints, to prevent noise, and for ordinary purposes of mechanical construction. It appeared possible that the expansion of the iron, due to heat, caused the plates to be pressed together more firmly when hot than when cold, and that this state of compression, if long continued, had a hardening effect on the iron similar to that caused by cold rolling or hammering.

One objection to this explanation was, that as the iron bolts holding the plates together were heated nearly as much as the plates, and therefore expanded nearly as much, the pressure could only vary slightly.

It was thought that in some forms such a gradual hardening might also be caused by the pressure exerted by the weight of the upper plates on the lower ones. But the increase was found where there was no superincumbent weight, in transformers in which the plates rested on their sides or edges.

In order to investigate this point two experiments were tried. The first was with the sample marked No. 5. In order, as far as possible, to avoid any compression, the plates in this sample were not bolted or clamped together, but, as described above, were merely held together by a binding of insulating tape and by the layer of copper wire with which they were covered. The results given in Table V do not differ materially from those of Nos. 1, 2, and 3.

An observation which tends to indicate that pressure is not the cause of the change of condition was made some time ago. Different parts of the iron of a transformer, which had been in use some time, were examined, when it was found that the middle portions were rather more affected than the end portions. As the end portions, being most exposed, are least heated, and as in this instance all parts were subject to the same amount of pressure by the clamps or bolts, this observation does not support the supposition that pressure is the cause of the change. As, however, commercial iron varies in its hysteresis, even in the same sheet, the observation is of doubtful value

A second set of tests is that marked No. 6. In this case the iron used was of the same size as in Nos. 1, 2, and 3, but of smaller amount.

It was arranged with a testing coil in the same manner, but instead of keeping the iron with its coil complete throughout the experiment, the coil was only put on for the tests, and was then removed, the iron being placed in the oven, arranged as a flat block (its upper surface being about 150 sq. ins.), with a weight of about 300 lbs. on it till the next test was made. It was thought that perhaps the slight pressure produced in this way might affect the results. These are given in Table VI and Diagram No. 6, and are markedly different from the tests of Nos. 1, 2, 3, and 5. The increase of hysteresis is greater, although the test has not been so long in progress.

This point deserves full investigation, and the author hopes to obtain further results from tests now in progress.*

Change of Power-factor.—The power-factor or ratio of watts to volts × ampères shows an irregular increase in all the tests. This increase in the power-factor indicates a possible (but improbable) increase in the eddies, such as would be accounted for by a decrease of the specific resistance of the iron. The author is proceeding to investigate this matter by continuously heating a ring formed by winding a volute of iron ribbon or wire, overwound with a testing coil of copper wire. The ends of the iron being brought out enable measurements of the resistance to be taken, the magnetising losses being measured as in the other samples.*

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* See Appendix I, p. 240.

Magnetisation not necessary to Production of the Effect.—Having ascertained that neither magnetic nor electric action was necessary to the production of this effect, the author has endeavoured to find whether alternate-current magnetisation of iron can be carried on continuously, or for considerable periods, without producing any noticeable change, and, although the evidence is not sufficient to justify a positive statement, he finds that so long as the iron is kept cool it suffers no reduction of permeability. For example, a transformer in regular use for certain testing purposes, and used several hours daily during the last fifteen months, has kept its permeability unaltered. It is magnetised at various densities up to about 2700 **B**. It is a specially designed apparatus, of large size for the work it has to do (and therefore inefficient and costly), and the temperature of its iron probably never rises more than about 6° to 8° C. above the room in which it is placed. It may be that its daily periods of rest account for the absence of change. This supposition is, however, not supported by other cases where iron which has been in regular use, but with daily periods of rest, has shown the increase of hysteresis very clearly.

Condition not Changed by Repose.—Further, the iron of some transformers which showed this increase, but which for several months have not been used, shows no return to the original condition. The author is therefore inclined to believe that if periods of repose in some cases prevent the rise, it is because the time of use is not long enough to allow of much increase of temperature.

This part of the subject is being pursued, but for the present the author will only say that, so long as iron is only slightly warmed (even when that warmth is caused by alternate-current magnetisation) its permeability remains unaffected.

It may be that repose at a temperature near or below zero, Centigrade, or at a still lower temperature, would have some effect. It appears possible, since continued expansion due to moderate heat can bring about this permanent increase of hysteresis, that continued contraction by moderate cold may bring the iron back to its original condition, or even permanently increase its permeability. The author intends to investigate this point.

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* See Appendix II, p. 241.

The slow increase of hysteresis,[*] which is the subject of this communication, must not be classed with the immediate effect produced by the moderate heating of iron. When the iron of transformers is heated, in working, it at first absorbs less energy than when cool, probably because the increase in its resistance lessens the eddies set up in it. This is the case whether the heating is by ordinary means or by magnetic reversals. It is a temporary effect—the loss goes up again on cooling. If it is kept heated, a slow change takes place, the loss gradually increasing again, from the decrease of permeability caused by heat.[†] If the temporary reduction of the loss is entirely accounted for by the increased resistance reducing the eddies, it appears to afford a means of separating the losses caused by hysteresis proper, and those caused by eddies. The ordinary measurement of magnetising loss of course gives the sum of these two.

The increase of hysteresis shown in the tests, and resulting from the heating, is somewhat greater than the author has observed in transformers where the heat has been caused by the magnetic and electric losses; possibly this is because in the latter cases the temperature has been somewhat lower.

All the tests show an increase of loss, the maximum apparently not yet having been reached. The tests are being continued.

Conclusions.—The conclusions to which these observations lead, so far as they have gone, are:—

1. The effect is not fatigue of the iron caused directly by repeated magnetic reversals—it is not "progressive magnetic fatigue."
2. Neither magnetic nor electric action is necessary to its production.
3. It is a physical change resulting from long-continued heating at a very moderate temperature.
4. It appears to be greater if pressure is applied during heating.
5. It is not produced when the iron is not allowed to rise more than a few degrees above the ordinary atmosphere.
6. It is similar to the effect produced by hammering, rolling, or by heating to redness and cooling quickly.
7. The iron returns to its original condition on re-annealing.
8. It does not return to its original condition if kept unused and at ordinary atmospheric temperatures, whether the periods of rest are short or long.

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[*] This effect may have an important influence on the reliability of measuring instruments having iron portions magnetised by alternate currents. The constants of such instruments may gradually fall.

[†] See Appendix III, p. 242.

Table I (see also Diagram 1).

Magnetisation = 2500 **B**. Section of Iron, 49·7 sq. cm.

	Ampères.	Power-factor.	Watts.	Date.
	0·41	0·74	16·54	August 24, 1894.
	0·50	0·75	20·76	September 20, „
	0·53	0·87	25·65	„ 27, „
	0·54	0·88	26·6	October 5, „
	0·56	0·83	25·71	„ 12, „
	0·58	0·85	26·9	„ 23, „
	0·59	0·89	28·77	„ 29, „
	0·60	0·81	26·71	November 5, „
	0·60	0·84	27·4	„ 13, „
	0·59	0·82	26·31	December 6, „
	0·61	0·85	28·88	„ 12, „
*	0·62	0·85	29·03	„ 27, „
	0·61	0·81	27·9	January 7, 1895.
	0·62	0·81	27·3	„ 25, „
	0·62	0·85	28·96	February 7, „

* Added since diagram was engraved.

DIAGRAM 1.

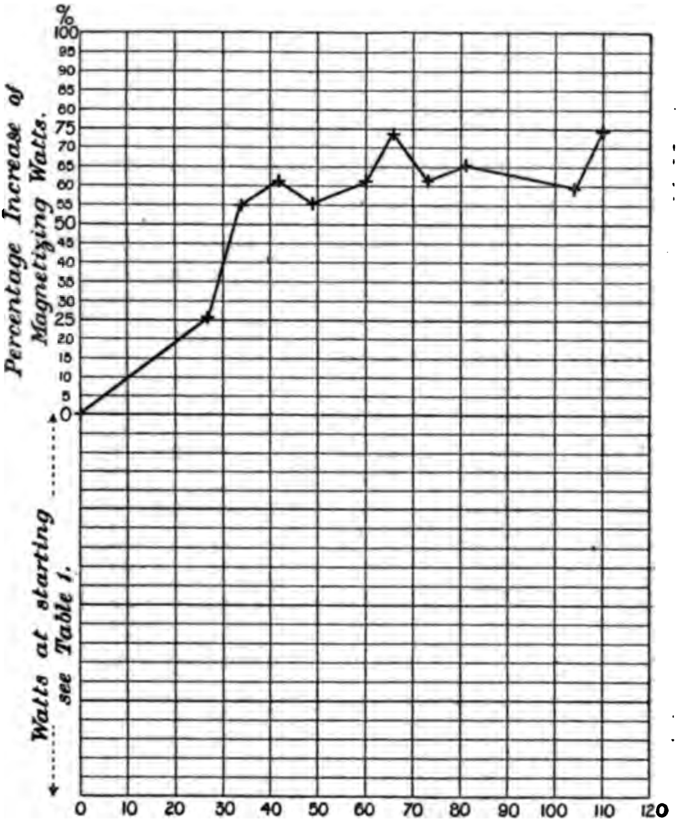

%
100
95
90
85
80
75
70
65
60
55
50
45
40
35
30
25
20
15
10
5
0
Percentage Increase of Magnetizing Watts.
Watts at starting see Table 1.
0
10
20
30
40
50
60
70
80
90
100
110
120
Days.

Table II (see also Diagram 2).

Magnetisation = 2500 **B**. Section of Iron, 49·7 sq. cm.

	Ampères.	Power-factor.	Watts.	Date.
	0·55	0·7	20·12	August 27, 1894.
	0·66	0·71	25·8	September, 20, „
	0·66	0·84	30·75	„ 27, „
	0·67	0·86	30·55	October 5, „
	0·71	0·75	28·79	„ 12, „
	0·70	0·8	30·99	„ 23, „
	0·71	0·85	33·08	„ 29, „
	0·71	0·8	30·05	November 5, „
	0·714	0·8	31·46	„ 13, „
	0·7	0·79	29·3	December 6, „
	0·74	0·745	32·54	„ 12, „
*	0·72	0·87	34·7	„ 27, „
	0·73	0·77	30·85	January 7, 1895.
	0·74	0·75	30·8	„ 25, „
	0·74	0·8	32·87	February 7, „

* Added since diagram was engraved.

Diagram 2.

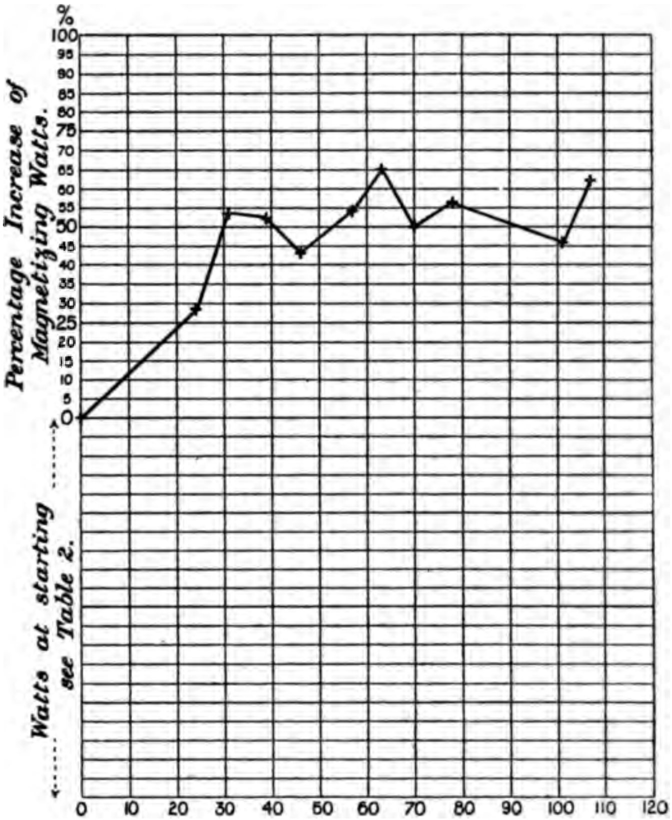
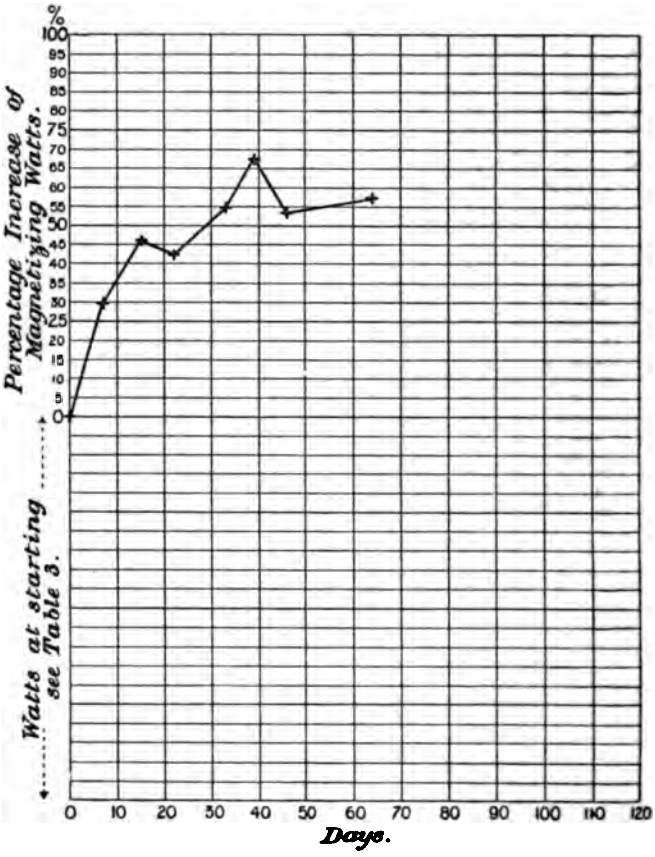

%
100
95
90
85
80
75
70
65
60
55
50
45
40
35
30
25
20
15
10
5
0
Percentage Increase of Magnetizing Watts.
Watts at starting see Table 2.
0
10
20
30
40
50
60
70
80
90
100
110
120
Days.

Table III (see also Diagram 3).

Magnetisation = 2500 **B**. Section of iron, 49·7 sq. cm.

Ampères.	Power-factor.	Watts.	Date.
0·43	0·7	16·61	September 20, 1894.
0·46	0·8	21·5	„ 27, „
0·51	0·86	24·28	October 5, „
0·53	0·8	23·7	„ 12, „
0·55	0·84	25·61	„ 23, „
0·56	0·89	27·67	„ 29, „
0·56	0·82	25·4	November 5, „
0·55	0·8	26·16	„ 13, „
Coil short circuited		..	December 6, „

Diagram 3.



This sample was then removed from the oven and left in repose. was again tested on January 25, 1895, with the following result:— Loss at 2500 B, 25·4 watts, showing that repose for fifty days d produced no effect.

Table V (see also Diagram 5).

Ampères.	Power-factor.	Watts.	B.	Date.
0·46	0·76	8·9	2500	September 29, 1894.
0·48	0·85	10·3	2762	
0·50	0·86	12·1	3050	
0·46	0·83	8·81	2500	October 5, ,,
0·48	0·8	9·91	2762	
0·51	0·84	12·08	3050	
0·49	0·79	8·85	2500	,, 12, ,,
0·51	0·78	80·18	2762	
0·53	0·81	12·18	3050	
0·56	0·86	11·15	2500	,, 23, ,,
0·59	0·86	12·82	2762	
0·61	0·87	14·79	3050	
0·58	0·88	11·73	2630	,, 29, ,,
0·60	0·9	14·9	2762	
0·61	0·92	15·94	3050	
0·60	0·88	12·13	2500	November 5, ,,
0·61	0·9	14·22	2762	
0·64	0·87	15·67	3050	
0·60	0·9	12·35	2500	,, 13, ,,
0·62	0·91	14·5	2762	
0·65	0·88	16·18	3050	
0·65	0·87	13·1	2500	December 6, ,,
0·69	0·88	15·5	2762	
0·71	0·88	17·68	3050	
0·67	0·9	14·28	2500	,, 12, ,,
0·70	0·95	16·96	2762	
0·72	0·92	18·76	3050	
* 0·64	0·9	13·83	2500	,, 27, ,,
0·67	0·9	15·74	2762	
0·71	0·9	18·37	3050	
0·67	0·82	12·7	2500	January 7, 1895.
0·69	0·86	15·1	2762	
0·72	0·84	17·0	3050	

* Added since diagram was engraved.

Table V—*continued.*

Ampères.	Power-factor.	Watts.	B.	Date.
* 0·67	0·83	12·9	2500	January 25, 1895.
0·71	0·81	14·64	2762	
0·74	0·81	16·32	3050	
0·69	0·87	13·9	2500	February 7, „
0·72	0·87	16·0	2762	
0·74	0·88	18·4	3050	

DIAGRAM 5.

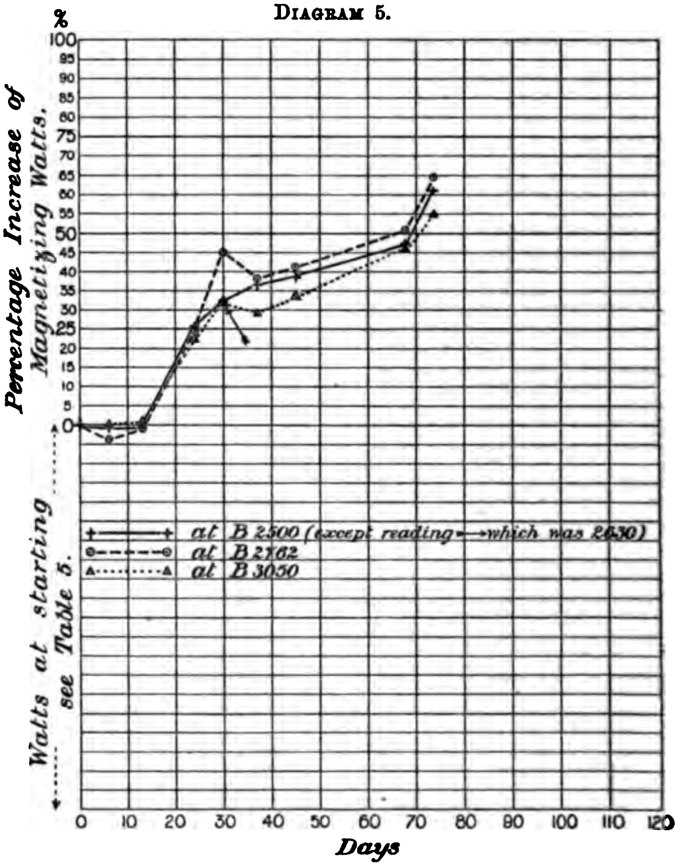


Table VI (see also Diagram 6).

Magnetisation = 2500 **B**.

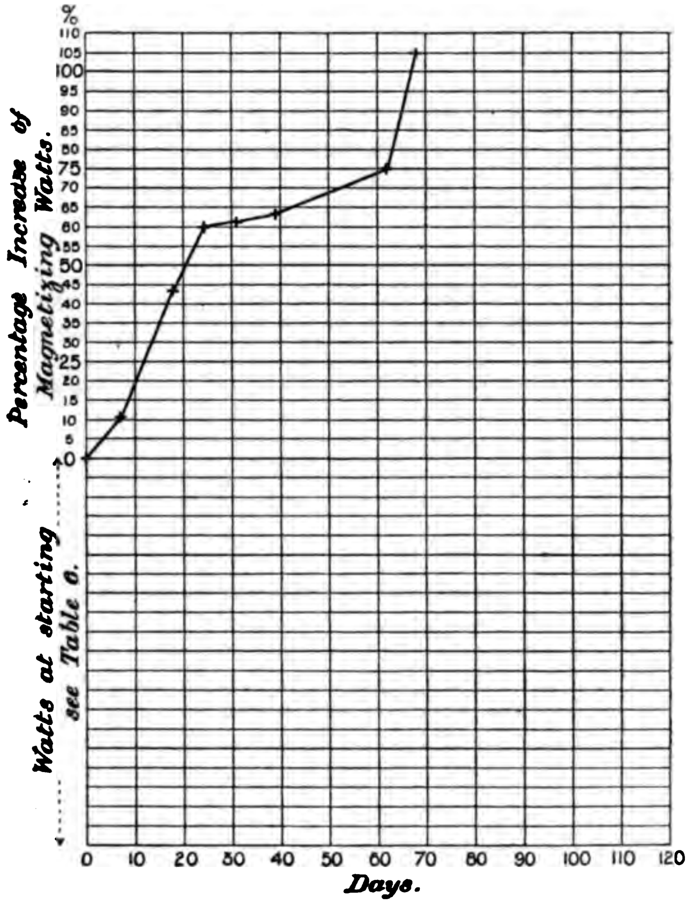
Ampères.	Power-factor.	Watts.	Date.
0·37	0·71	7·25	October 5, 1894.
0·38	0·77	7·99	„ 12, „

* Added since diagram was engraved.

Table VI—*continued.*

	Ampères.	Power-factor.	Watts.	Date.		
	0·46	0·8	10·33	October	23,	1894.
	0·46	0·9	11·55	„	29,	„
	0·50	0·84	11·61	November	5,	„
	0·59	0·85	11·8	„	13,	„
	0·55	0·82	12·63	December	6,	„
	0·56	0·94	14·8	„	12,	„
*	0·596	0·86	14·2	„	27,	„
	0·586	0·80	13·0	January	7,	1895.
	0·61	0·76	12·65	„	25,	„
	0·616	0·82	13·9	February	7,	„

DIAGRAM 6.



* Added since diagram was engraved.

Added February 9, 1895.

APPENDIX I.

Effect of Pressure without Heat.—The following test on the effect of pressure has been made without the application of heat. A transformer constructed in the manner of Fig. 1 was taken and tightened up by hand in the usual way, sufficient for ordinary purposes of construction. The loss, on magnetising to 2180 **B** at 100 ~ was found to be 59·8 watts.

It was then submitted to a pressure of 100 tons, or about 1500 lbs. per sq. in. (10·57 kilos. per sq. cm.), in a direction to force the laminæ closer together, the tightening bolts then being screwed up with the pressure on. On magnetising again to 2180 **B** the loss was found to be 72·5 watts, or an increase of 21 per cent.

It was kept bolted up in this way for 30 days, and readings taken occasionally as shown in Table IV, the loss remaining practically unchanged under this continued application of pressure.

The bolts were then slackened and the loss, on being again measured, was found to be the same as before the pressure was applied.

Thus, without heat, pressure (up to the limits of the test) produces no permanent effect. But that even a very small pressure will increase the permanent effect of heat is shown by the results given in Table VI.

It should, however, be remembered that Table VI is the record of only a single set of tests, and that it is not known what the effect would have been if the iron had been heated without any application of pressure. It is, therefore, safe only to say that under considerable pressure, without the application of heat, there is a change of a definite amount which remains constant under constant pressure, and which disappears immediately on the removal of the pressure.[*]

It is, however, clear that the slow decrease in permeability under continued moderate heating is not to be accounted for by hardening produced by pressure.

The hardening, which takes place very slowly at these low temperatures, is similar, apparently, to the hardening which takes place when iron is heated to a high temperature and then suddenly cooled.

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[*] Experiments on a solid bar under pressure have been made by Ewing and Low. "On the Influence of a Plane of Transverse Section on the Magnetic Permeability of an Iron Bar," 'Phil. Mag.,' September, 1888.

Table IV.

(Without application of heat; magnetised only for a few minutes occasionally for purpose of measuring loss.)

	Watts.
Before applying pressure......	59·8
During application of pressure of 1500 lbs. per sq. in. (10·57 kilos. per sq. cm.)	72·5 on application. 70·2 after 9 days. 72·2 „ 22 „ 70·2 „ 30 „
Without pressure	59·4 on 30th day.

The author has a test in hand to find the effect of heat combined with considerable pressure, but cannot yet give any results.

APPENDIX II.

The results obtained in this way, so far as the investigation has gone, are as follows :—

A ring was made up of iron ribbon, wound in a close volute, the convolutions separated from each other by paper. This was then wound with a magnetising winding of copper wire covering the whole ring. A test was taken on January 15, 1895, of the resistance of the iron, and of the energy spent in magnetising it to 2500 **B** at 100 ∼. It was then kept in an oven at a temperature ranging between 60° and 75° C. It was taken out, cooled, and the tests repeated on January 25 and February 8 as below :—

Date.	Temperature.	Watts at 100 ∼.	Power-factor.	Resistance of the iron.
	C°.			
January 15..........	17·3	4·9	0·72	0·94^{10}
„ 25..........	17·0	5·4	0·73	0·96
February 8..........	17·3	6·7	0·8	1·07

Thus the hysteresis loss has gone up considerably, as a result of continued heating, and the resistance has also risen. This result is in accordance with what is known as to the connection between the magnetic permeability and the electrical resistance of iron. Professor Hughes showed many years ago* that the lower the resistance of iron the better its magnetic qualities.

As bearing on this point, the author has measured the resistance of some soft annealed iron strips, which were then hardened by being heated to redness and quenched in oil at 14° C. It was found that this hardening process increased the resistance 4 per cent. and 7 per cent. in different samples.

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* 'Roy. Soc. Proc.,' December, 1883.

Appendix III.

This effect may be illustrated by an example. A transformer was kept in a tank of heated oil, the temperature of which was kept between 110° and 140° C. Readings of its loss were taken occasionally as below, the magnetisation being the same in all cases, the current being applied only at the time and for the purpose of the test.

This test has not yet been in progress long enough to show the slow rise very clearly.

	Watts.	
On immersion	33·13	Jan. 1, 1895.
After 20 minutes	32·19	
„ 40 „	32·1	
„ 1 hour	32·0	
„ 1 „ 20 minutes ..	32·0	
„ 2 hours 30 „ ..	31·54	
„ 24 „	28·88	
„ 2 days	29·6	
„ 3 „	30·67	
„ 4 „	31·5	
„ 6 „	36·0	
„ 7 „	33·6	
„ 8 „	36·6	
„ 9 „	32·6	
„ 18 „	32·9	
„ 29 „	35·9	Jan. 29, 1895.

Presents, January 17, 1895.

Transactions.

Adelaide:—Royal Society of South Australia. Transactions and Proceedings. Vol. XVIII, for 1893–94. 8vo. *Adelaide* 1894. The Society

Baltimore:—Johns Hopkins University. Circulars. Vol. XIV No. 115. 4to. *Baltimore* 1894. The University

Berlin:—Gesellschaft für Erdkunde. Verhandlungen. Bd. XXI No. 9. 8vo. *Berlin* 1894; Zeitschrift. Bd. XXIX. No. 5 8vo. *Berlin* 1894. The Society

Brünn:—Naturforschender Verein. Verhandlungen. Band XXXI 1893. 8vo. *Brünn* 1894. The Society

Brussels:—Académie Royal de Médecine. Mémoires Couronné Tome XIII. 8vo. *Bruxelles* 1894. The Academ

Transactions (*continued*).

Buda-Pesth :—Königl. Ungarische Geologische Anstalt. Földtani Közlöny. Kötet XXIV. Füzet 6—10. 8vo. *Budapest* 1894; Mittheilungen. Bd. X. Heft 6. 8vo. *Budapest* 1894.
The Institute.

Buitenzorg :—'s Lands Plantentuin. Mededeelingen. No. XIII. 8vo. *Buitenzorg* 1894. The Director.

Calcutta :—Indian Museum. Catalogue of the Coins. Part 1. 8vo. *Calcutta* 1894. The Trustees.

Cambridge, Mass. :—Museum of Comparative Zoölogy. Bulletin. Vol. XXV. No. 11. 8vo. *Cambridge, Mass.* 1894.
The Museum.

Copenhagen :—K. Danske Videnskabernes Selskab. Oversigt. 1894. No. 2. 8vo. *Kjøbenhavn.* The Society.

Cork :—Queen's College. Calendar. Session 1894–95. 12mo. *Cork* 1894. The College.

Cracow :—Académie des Sciences. Bulletin International. Novembre, 1894. 8vo. *Cracovie.* The Academy.

Dresden :—Verein für Erdkunde. Jahresbericht. 24. 8vo. *Dresden* 1894; Litteratur der Landes- und Volkskunde des Königreichs Sachsen. Nachtrag 2. 8vo. *Dresden* 1894.
The Society.

Edinburgh :—Royal College of Physicians. Reports from the Laboratory. Volume 5. 8vo. *Edinburgh* 1894.
The College.

Scottish Microscopical Society. Proceedings. Session 1893–94. 8vo. *Edinburgh* 1894. The Society.

Genoa :—Società Ligustica di Scienze Naturali e Geografiche. Vol. V. No. 3. 8vo. *Genova* 1894. The Society.

Helsingfors :—Sällskapet för Finlands Geografi. Fennia. Parts 9, 11. 8vo. *Helsingfors* 1894. The Society.

Irkutsk :—East Siberian Section, Imperial Russian Geographical Society. Izvestiya. [*Russian.*] Tom. XXV. No. 1. 8vo. *Irkutsk* 1894. The Section.

Kazan :—Imperial University. [Two Theses. *Russian.*] 8vo. *Kazan* 1894; [Scientific Notes. *Russian.*] 1894. No. 6. 8vo. *Kazan.* The University.

Kew :—Royal Gardens. Bulletin of Miscellaneous Information. No. 95. 1894. 8vo. *London* 1894. The Director.

Leipsic :—Astronomische Gesellschaft. Catalog. Abth. I. Stück 6. 4to. *Leipzig* 1894; Vierteljahrsschrift. Jahrgang 29. Heft 3. 8vo. *Leipzig* 1894. The Society.

Königl. Sächs. Gesellschaft der Wissenschaften. Berichte über die Verhandlungen. Math. Phys. Classe. 1894. Part 2. 8vo. *Leipzig* 1894. The Society.

Transactions (*continued*).

Liverpool:—Liverpool Geological Society. Proceedings. Session XXXV. Volume 7, Part 2. 8vo. *Liverpool* 1894. The Society.

London:—British Astronomical Association. Journal. Vol. V. No. 2. 8vo. *London* 1894. The Association.

Camera Club. Journal. Vol. VIII. No. 103. 8vo. *London* 1894; Vol. IX. No. 104. 8vo. *London* 1895. The Club.

Entomological Society. Transactions. 1894. Part 4. 8vo. *London* 1894. The Society.

Odontological Society. Transactions. Vol. XXVII. No. 1. 8vo. *London* 1894. The Society.

Quekett Microscopical Club. Journal. Vol. V. No. 35. 8vo. *London* 1894. The Club.

Royal Medical and Chirurgical Society. Transactions. Volume 77. 8vo. *London* 1894. The Society.

Royal Microscopical Society. Journal. 1894. Part 6. 8vo. *London* 1894. The Society.

Royal Photographic Society. Journal and Transactions. Vol. XIX. No. 4. 8vo. *London* 1894. The Society.

Royal Statistical Society. Journal. Vol. LVII. Part 4. 8vo. *London* 1894. The Society.

Royal United Service Institution. Journal. Vol. XXXVIII. No. 202. 1894. 8vo. *London* 1894. The Institution.

Magdeburg:—Naturwissenschaftlicher Verein. Jahresbericht und Abhandlungen. 1893–94. 8vo. *Magdeburg* 1894; Festschrift zur Feier des 25 jährigen Stiftungstages. 8vo. *Magdeburg* 1894. The Society.

Manchester:—Literary and Philosophical Society. Memoirs and Proceedings. Vol. 8. No. 4. 8vo. *Manchester* 1894. The Society.

Manchester Geological Society. Transactions. Vol. XXIII. Parts 1, 2. 8vo. *Manchester* 1894. The Society.

Naples:—Accademia di Archeologia, Lettere e Belle Arti. Anno VIII. Marzo-Giugno. 8vo. *Napoli* 1894. The Academy.

Newcastle-upon-Tyne:—Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne. Transactions. Vol. XI. Parts 1, 2. 8vo. *Newcastle-upon-Tyne* 1892–94. The Society.

North of England Institute of Mining and Mechanical Engineers. Transactions. Vol. XLIII. Parts 5, 6. Vol. XLIV. Part 1. 8vo. *Newcastle-upon-Tyne* 1893-94; Report of the Proceedings of the Flameless Explosives Committee. Part 1. 8vo. *Newcastle-upon-Tyne* 1894; Annual Report of the Council 1893–94. 8vo. *Newcastle-upon-Tyne* 1894. The Institute.

Transactions (*continued*).

New York:—American Museum of Natural History. Bulletin. Vol. VI. Pages 321—336. 8vo. [*New York*] 1894; Vol. VI. Pages 337—352. 8vo. [*New York*] 1894. The Museum.

Palermo:—Circolo Matematico. Rendiconto. Tome VIII. Fasc. VI. Anno 1894. 8vo. *Palermo* 1894. Circolo Matematico.

Paris:—École Normale Supérieure. Annales Scientifiques. Série 3. Tome XI. No. 12. 4to. *Paris* 1894; Table des Matières contenues dans les Dix Premiers Volumes de la 3e Série. 4to. *Paris* 1894. The School.

Société de Géographie. Bulletin. Tome XV. Trimestre 3. 1894. 8vo. *Paris* 1894. The Society.

Société de Mathématique de France. Bulletin. Tome XXII. No. 9. 8vo. *Paris* (1894). The Society.

Société Française de Physique. Séances. Année 1894. Fascicule 3. 8vo. *Paris* 1894. The Society.

Rome:—Pontificia Accademia dei Nuovi Lincei. Memorie. Vols. VII, VIII. 8vo. *Roma* 1891–92. The Academy.

St. Petersburg:—Académie Impériale des Sciences. Bulletin. Série 5. Volume I. 1894. 4to. *St. Petersburg* 1894; Mémoires. Tome XLI. No. 7. Tome XLII. Nos. 7 and 8. 4to. *St. Petersburg* 1893-94. The Academy.

Santiago:—Sociedad Nacional de Mineria. Boletin. Año XI. No. 70. 4to. *Santiago de Chile* 1894. The Society.

Shanghai:—Royal Asiatic Society. (China Branch.) Journal. Vol. XXVI. 1891–92. 8vo. *Shanghai* 1894. The Society.

Siena:—R. Accademia dei Fisiocritici. Atti. Vol. VI. Fasc. 10. 8vo. *Siena* 1894; Processi Verbali delle Adunanze. No. 6. 8vo. *Siena* 1894. The Academy.

Stockholm:—Kongl. Vetenskaps-Akademie. Öfversigt. Årg. LI. No. 8. 8vo. *Stockholm* 1894. The Academy.

Throndhjem:—Kong. Norske Videnskabers Selskabs. Skrifter. 1892. 8vo. *Throndhjem* 1893. The Society.

Turin:—Reale Accademia delle Scienze. Memorie. Tomo XLIV. 4to. *Torino* 1894. The Academy.

Vienna:—Kais. Akademie der Wissenschaften. Sitzungsberichte (Math.-naturw. Classe). Bd. CIII. Abth. 2a. Heft 8. 8vo. *Wien* 1894. The Academy.

K.K. Geologische Reichsanstalt. Verhandlungen. 1894. Nos. 10—13. 8vo. *Vienna* 1894. The Institute.

Würzburg:—Physikalisch-medicinische Gesellschaft. Verhandlungen. Bd. XXVIII. Nrs. 2—5. 8vo. *Würzburg* 1894; Sitzungs-Berichte. Jahrg. 1894. Nrs. 5, 6, 7. 8vo. *Würzburg* 1894. The Society.

Observations and Reports.

Buda-Pesth:—Königl. Ung. Central-Anstalt für Meteorologie, &c. Jahrbücher. Band XXI. Jahrgang 1891. 8vo. *Budapest* 1894. The Institute.

Brünn:—Naturforschender Verein. Bericht der Meteorologischen Commission. Jahre 1892. 8vo. *Brünn* 1894. The Society.

Calcutta:—Meteorological Department. Monthly Weather Review. June, 1894. 4to. *Calcutta;* Meteorological Observations made at Seven Stations in India. June, 1894. 4to. [*Calcutta.*] The Department.

India:—Geological Survey. Records. Vol. XXVII. Part 4. 8vo. *Calcutta* 1894. The Survey.

Leyden:—Sterrenwacht. Verslag van den Staat der Sterrenwacht. 1893-94. 8vo. *Leiden* 1894. The Observatory.

London:—Hydrographic Department, Admiralty. Longitude of Penang, Straits Settlements, determined by means of Telegraphic Signals exchanged with Singapore on 30th and 31st December, 1893. Folio. *London* 1894. The Hydrographer.

Madras:—Archæological Survey of India. (New Imperial Series.) Vols. XV and XVII. 4to. *Madras* 1894. The Survey.

Paris:—Bureau des Longitudes. Annuaire pour l'an 1895. 12mo. *Paris* 1894. The Bureau.

San Salvador:—Observatorio Astronómico y Meteorológico. Observaciones Meteorológicas, 1892. 8vo. *San Salvador* [1894]. The Observatory.

Sydney:—Observatory. Meteorological Observations. July, 1894. 8vo. [*Sydney*] 1894. The Observatory.

Washington:—Nautical Almanac Office. Extract from the Annual Report of the Superintendent. 1894. 8vo. *Washington.* The Superintendent.

U.S. Department of Agriculture. Experiment Station Record. Vol. V. Nos. 10, 11. 8vo. *Washington* 1894. The Department.

Zurich:—Schweizerische Meteorologische Central-Anstalt. Annalen. 1892. 4to. *Zürich* [1894]. The Institute.

Journals.

Agricultural Gazette of New South Wales. Vol. V. Part 10. 8vo. *Sydney* 1894. Department of Agriculture, Sydney.

Anales de la Asociacion de Ingenieros y Arquitectos. Tomo IV. Entregas 1—3. 8vo. *México* 1894. The Editors.

Journals (*continued*).

Archives Néerlandaises des Sciences Exactes et Naturelles. Tome XXVIII. Livr. 3, 4. 8vo. *Harlem* 1894.
Société Hollandaise des Sciences.

Astronomische Nachrichten. Band CXXXVI. 4to. *Kiel* 1894.
The Observatory, Kiel.

Canadian Record of Science. Vol. V. No. 8. 8vo. *Montreal* 1893.
Natural History Society, Montreal.

Morphologisches Jahrbuch. Bd. XXII. Heft 2. 8vo. *Leipzig* 1894.
Prof. Gegenbaur, For. Mem. R.S.

Naturalist. December, 1894. 8vo. *Leeds*; January, 1895. 8vo. *Leeds.*
The Editors.

Nature Notes. Vol. V. No. 60. 8vo. *London* 1894; Vol. VI. No. 61. 8vo. *London* 1895.
Selborne Society.

Physical Review. Vol. II. Nos. 1, 2. 8vo. *New York* 1894.
The Editors.

Stazioni Sperimentali Agrarie Italiane. Vol. XXVII. Fasc. 3. 8vo. *Modena* 1894; Vol. XXVII. Fasc. 4. 8vo. *Modena* 1894.
R. Stazione Agraria, Modena.

Zeitschrift für Biologie. Bd. XXXI. Heft 4. 8vo. *München* 1894.
The Editors.

Zeitschrift für Naturwissenschaften. Band 67. Hefts 3 and 4. 8vo. *Leipzic* 1894.
Naturwissenschaftlicher Verein, Halle.

Acland (Sir Henry W., F.R.S.) The Unveiling of the Statue of Sydenham in the Oxford Museum, August 9, 1894, by the Marquess of Salisbury, K.G., Chancellor of the University, &c., with an address by Sir Henry W. Acland. 8vo. *Oxford* 1894.
The Author.

Alcock (A.) and Anderson (A. R.) Natural History Notes from H.M. Indian Marine Survey Steamer "Investigator." Series II. No. 17. 8vo. *Calcutta* 1894.
The Authors.

Dubois (E.) Pithecanthropus erectus: eine menschenaehnliche Uebergangsform aus Java. 4to. *Batavia* 1894.
The Colonial Minister, The Hague.

Du Hamel (J. B.) Opera Philosophica. 2 vols. in 1. 4to. *Norimbergæ* 1681.
The Linnean Society.

Gaudry (A.) Les Pythonomorphes de France. 4to. *Paris* 1892.
The Author.

Gibbs (W.) Researches on the Complex Inorganic Acids. Part 6. 8vo. *Cambridge* [*Mass.*] 1894.
The Author.

Issaly (Abbé). Optique Géométrique: Genèse, Variété, et Polarisation Axiale des Faisceaux de Rayons Lumineux ou Calorifiques. 8vo. *Bordeaux* [1894].
The Author.

Loescher (M. G.) Physica Experimentalis Compendiosa. 8vo. *Vitembergae* 1715. The Linnean Society.

Lubbock (R.) Dissertatio Physico-Chemica, inauguralis, de Principio Sorbili, sive communi Mutationum Chemicarum Causa, Quaestionem, an Phlogiston fit Substantia, an Qualitas, agitans. 8vo. *Edinburgi* 1784. The Linnean Society.

Naber (H. A.) Standard Methods in Physics and Electricity criticised, and a Test for Electric Meters proposed. 8vo. *London* 1894. The Author.

Schreiber (P.) Meteorological Observations considered with Special Reference to influence on Vegetation. 8vo. *Chicago* 1894. The Author.

"Secrets." Hundert acht und dreyszig neu-Entdeckte und volkommen bewährte Geheimnüsse, oder allerhand magische . . . . Kunst-Stücke. 12mo. *Franckfurt* 1726. The Linnean Society.

Spencer (J. W.) Deformation of the Lundy Beach and Birth of Lake Erie. 8vo. *New Haven* 1894; The Duration of Niagara Falls. 8vo. *New Haven* 1894. The Author.

Walker (R.) An Account of some Remarkable Discoveries in the Production of Artificial Cold; with Experiments on the Congelation of Quicksilver in England. 8vo. *Oxford* 1796. The Linnean Society.

January 24, 1895.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Right Hon. Horace, Lord Davey, a member of Her Majesty's Most Honourable Privy Council, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "Notes of an Enquiry into the Nature and Physiological Action of Black-damp, as met with in Podmore Colliery, Staffordshire, and Lilleshall Colliery, Shropshire." By JOHN HALDANE, M.A., M.D., Lecturer in Physiology, University of Oxford. Communicated by Professor BURDON SANDERSON, F.R.S. Received December 6, 1894.

Black-damp, sometimes also called choke-damp, or "stythe," is one of the gases frequently found in the workings of coal mines. It is distinguished from fire-damp by the fact that it is not explosive when mixed with air, but extinguishes flame; and from after-damp by the fact that it is not the product of an explosion, but collects in the workings under ordinary conditions. Like after-damp and fire-damp, it produces fatal effects when inhaled in sufficient concentration. A further distinction has been drawn between black-damp and white-damp, which latter is described as capablo of supporting combustion, while at the same time acting as a poison when inhaled.

Black-damp usually occurs in old workings and other ill-ventilated parts of coal mines. It is much more common in some districts than in others. As to its composition, very little seems to have been hitherto ascertained, and I have not been able to discover any published analysis. The prevailing opinion, however, is that it consists of carbonic acid, and that its suffocative properties are due to this gas.* This opinion appears to be chiefly based on the observation that black-damp frequently lies along the floors of workings with fresh air above it, and must therefore be, like carbonic acid, heavier than air.

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* See, for instance, minutes of evidence before the "Royal Commission on Accidents in Mines," Nos. 46, 768, 6930, 11702-3, 11707-14.

In a paper read before the British Association in August of the present year* I expressed the opinion that black-damp is chiefly nitrogen, and does not contain more than from 10 to 20 per cent. of carbonic acid. One of the grounds for this opinion was the fact that the symptoms and the action on lamps described as produced by diluted black-damp do not correspond with those produced by mixtures of carbonic acid with air.

Through the kind co-operation of Mr. W. N. Atkinson, H.M. Inspector of Coal Mines, I have lately had the opportunity of making observations on concentrated black-damp from two pits, the first being in a fiery and the second in a non-fiery district. These two pits were selected by Mr. Atkinson as affording characteristic specimens of black-damp. In each case the gas was obtained from unused workings through an iron pipe inserted through a stopping. On turning the tap with which the pipe was provided, the black-damp issued in an abundant stream.

Specimens were collected in the following manner:—A short piece of rubber tubing was attached to the tap by means of a cork perforated by glass tubing. The other end of the rubber tubing was attached to another doubly-tubulated cork, which was fitted into a small glass bottle. The second tube of this cork was connected by rubber tubing with a small syringe, provided with valves arranged for aspirating through the bottle. With the help of the syringe, the bottle was washed out with about twenty times its volume of gas from the tap, and then, while held mouth upwards, at once closed perfectly air-tight by forcing into it a cork which had been previously boiled in somewhat soft paraffin wax. I adopted this method owing to the impossibility of using naked lights in a fiery mine for the purpose of sealing up specimens of gas in glass tubes.

Podmore Colliery, Staffordshire.

An ordinary locked safety-lamp held under the tap was at once extinguished when the black-damp was allowed to issue, as was a Clowes hydrogen lamp. Two other safety-lamps, which had been put down or hung up a few yards away, were also extinguished while the specimens were being collected. I was provided with one of the small electric hand-lamps made by the Lithanode Company, which afforded an excellent light, so that the loss of the oil lights caused no inconvenience. After the specimens had been collected, the tap was arranged so as to enable me to inhale the black-damp as it issued, without dilution. I inspired from the tap and expired through the nostrils. The sensation produced at the back of the mouth was identical with that produced by air containing 8 or 10 per cent. of carbonic acid. There was no smell of sulphuretted hydrogen or any other gas. Two or three inspirations were sufficient to produce marked increase in the depth and frequency of the respirations. The hyperpnœa rapidly became more intense and was accompanied by palpitations. At the same time I was observed to become blue in the face. Within about thirty seconds I felt that I was becoming confused, and did not push the observations further. Two or three seconds after I had ceased to breathe from the pipe the blueness was seen suddenly to disappear from my face and lips, and to be replaced by the natural colour.

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* An abstract of the paper appeared in the "Times" of August 18, 1894.

The feelings which I experienced after the first fifteen or twenty seconds in breathing the black-damp were distinctly such as are produced by breathing air very poor in oxygen. From previous experiments I had become familiar with the distinction between the feelings produced by excess of carbonic acid and those due to deficiency of oxygen. The experiment of breathing the black-damp was repeated twice in order to leave no doubt. An analysis of the black-damp gave the following result:—

Nitrogen	82·56
Oxygen	1·45
Carbonic acid	10·64*
Methane	5·35
Total	100·00

I tested part of the mixture for carbonic oxide by shaking with a little very dilute blood. Neither with the spectroscope nor by the change of tint could I discover any evidence of the presence of carbonic oxide. The test employed would have detected 0·05 per cent. The oxygen was determined by absorption with alkaline pyrogallate solution of the composition recommended by Hempel. The methane was determined by explosion in a Hempel's explosion pipette, after the addition of oxygen and detonating gas. All the measurements were made over mercury. The gas-burette was a modification of that of Petterson.

The calculated specific gravity of this mixture is 1·0106, or very nearly that of air. The extra nitrogen and the methane are both considerably lighter than the oxygen which they replace, and thus counterbalance the effect on the specific gravity of the heavier carbonic acid.

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* A second determination of the carbonic acid gave exactly the same result (10·64).

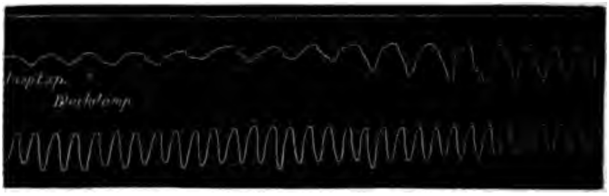
Lilleshall Colliery, Shropshire.

The part of the heading nearest the pipe had, at Mr. Atkinson's request, been bratticed (curtained) off, so that a chamber was provided in which the black-damp could be allowed gradually to accumulate. The heading was about 3½ ft. high. As the mine is not a fiery one, it was possible to use naked lights.

The gas from the pipe at once extinguished a candle. On breathing the gas I found that my respirations rapidly became deeper and more frequent, so that after a short time I was panting violently. The panting, however, did not increase beyond a certain point, and was not distinctly accompanied by the peculiar feelings characteristic of distress from want of oxygen. After breathing the black-damp for two minutes it seemed to me that the distress was, if anything, diminishing, and that I might have continued to breathe from the pipe for a considerable time without injury. The panting was accompanied by blueness of the face, which did not, however, seem to Mr. Atkinson to be so marked as it was at the Podmore Pit, although the change of colour when I ceased to breathe from the pipe was quite distinct. A similar result was obtained when the gas was breathed by Mr. Richard Warren, who had accompanied me.

The effect of the black-damp on a mouse was then observed. The animal was placed in a piece of wide glass tubing, over one end of which gauze had been tied. The end of this tubing was then luted with clay to the opening of the pipe (which was directed upwards), and the gas allowed to pass through the gauze, on which the animal sat. It at once began to pant violently, but did not lose power over its limbs, or seem to become otherwise seriously affected, although exposed to the black-damp for eleven minutes. The panting did not increase after the first few seconds, and ceased at once when the animal was taken out. It was in no way injured by the experiment.

With Mr. Warren's help a graphic record was then taken, by means of a Marey's recording stethograph, of the effect of the black-damp on my respirations. The tracing obtained is reproduced below. An interval of two or three seconds (while the pen was being shifted) elapsed between the end of the upper tracing and the beginning of



the lower. The time-marker indicates seconds. It will be seen that the action of the black-damp became apparent after two inspirations and attained its maximum after twenty inspirations, or one minute, and that subsequently the hyperpnœa gradually became somewhat less marked.

After this observation the tap was turned full on and the brattice closed, two of the party remaining outside in case of any accident. A sample of air[*] taken at the moment when the last light (a candle fixed horizontally) was extinguished, had the following composition:—

Oxygen	17·05
Carbonic acid	2·62
Nitrogen	80·33
Total	100·00

or as compared with air.[†]

Diminution in oxygen percentage	3·85
Increase in carbonic acid percentage	2·59
„ nitrogen „	1·26

This composition corresponds to what would have been expected from previous observations. Thus, in experiments by Dr. Lorrain Smith and myself on air vitiated by respiration,[‡] a match could no longer be lighted when the air contained about 17·7 per cent. of oxygen and 2·5 per cent. of carbonic acid, or 17·4 per cent. of oxygen and 0·4 per cent. of carbonic acid.

About half an hour later, Mr. Warren's breathing had become somewhat deeper than usual, though my own was not distinctly affected. A sample of air taken at this point had the following composition:—

Oxygen	15·30
Carbonic acid	3·38
Nitrogen	81·32
Total	100·00

or as compared with air—

Diminution in oxygen percentage	5·60
Increase in carbonic acid percentage	3·35
„ nitrogen „	2·25

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[*] The sample was taken close to, and at the level of, the candle, about midway between the floor and the roof. An upright candle and a safety lamp had already gone out.

[†] With the same gas-burette, used under the same conditions, pure air gave 20·90 per cent. of oxygen.

[‡] 'Journal of Pathology and Bacteriology,' 1892.

Neither of us could detect any loss of power, headache, or any other abnormality, nor did we experience any trace of after-effect on coming out of the mine. We did not continue these observations further, as the vitiation was now evidently proceeding very slowly, owing to the slight ventilation through the brattice cloth. When this was lifted, the lights of the party outside were suddenly extinguished, so that the electric light alone remained; and for some time it was impossible to re-light a candle.

The gas from the pipe had the following composition :—

Oxygen	9·60
Carbonic acid	7·32
Nitrogen..................	83·08
Total	100·00

or as compared with air—

Diminution in oxygen percentage	11·30
Increase in carbonic acid percentage	7·29
„ nitrogen „	4·01

I could detect no trace of carbonic oxide, or of any gas yielding carbonic acid on exploding the mixture, previously freed from carbonic acid, with detonating gas.

The calculated specific gravity of this mixture is 1·0258. A direct determination was also made. A dry flask was filled directly from the pipe, through drying tubes containing pumice and sulphuric acid. The weight of the contained mixture at atmospheric pressure and temperature was determined as against the weight of the same volume of dry air at the same pressure and temperature. A similar flask was used as a counterpoise in both weighings.

The specific gravity as found was 1·0252.

If we regard methane and air as accidental admixtures, and calculate the air from the oxygen, the composition of the two specimens of black-damp was as follows:—

		No. 1.	No. 2.
Methane		5·35	0·00
Air	oxygen	1·45	9·60
	nitrogen	5·49	36·33
	carbonic acid	0·00	0·01
Black-damp	nitrogen......	77·07	46·75
	carbonic acid..	10·64	7·31
Total		100·00	100·00

The undiluted black-damp had thus the following composition and specific gravity:—

	No. 1.	No. 2.
Nitrogen	87·87	86·48
Carbonic acid	12·13	13·52
Total	100·00	100·00
Specific gravity	1·0390	1·0468

This composition supports the view that black-damp is the residual gas left on slow oxidation of the carbon and hydrogen of coal by air. Supposing that the black-damp were produced by the oxidation of the carbon and hydrogen which would, under other conditions, be given off as methane, it would consist of 88·30 per cent. of nitrogen and 11·70 per cent. of carbonic acid. This composition very closely approximates to that actually found in the first specimen. The second specimen shows a larger proportion of carbonic acid, and corresponds to the oxidation of material containing about 3 atoms of hydrogen to one of carbon. It must be borne in mind, however, that the coal may itself supply part of the oxygen required for the formation of carbonic acid and water in the oxidation process.

The physiological action of black-damp such as that examined is due partly to its deficiency in oxygen, and partly to its carbonic acid. When mixed in increasing proportions with air, its action, as shown above, first becomes sensible when the oxygen is diminished to about 15 per cent. and the carbonic acid increased to about 3·5 per cent. From the observations previously made by Hermans, and by Lorrain Smith and myself,* it follows that the hyperpnœa which began to be produced at this point was due to excess of carbonic acid, and not to deficiency of oxygen. Until the carbonic acid is increased to about 7 per cent. the same inference holds good; the effect on the respiration is almost the same as that of air or oxygen containing 7 per cent. of carbonic acid; and the breathing of such air for some time is attended with no immediate risks to normal persons. When, however, the air is still further vitiated the deficiency in oxygen begins to tell dangerously; and when the oxygen is reduced to about 7 per cent. there is imminent risk of loss of sensation and power over the limbs. The danger-point from deficiency of oxygen (about 7 per cent.) is sooner reached in the case of a mixture of black-damp and air than the danger-point from excess of carbonic acid. The presence of the carbonic acid in black-damp would seem in reality to diminish the risk, since, by causing panting, it gives some warning of the impending real danger from deficiency of oxygen. Were there no carbonic acid in black-damp the danger would approach much more insidiously, as hyperpnœa, &c., are not caused by deficiency of oxygen until real danger is imminent. Were black-damp ever carbonic acid, or chiefly carbonic acid, as commonly supposed, the breathing would with increasing proportions certainly be markedly affected long before the point at which lights are extinguished.* I have not been able to obtain any account of gas met with in coal mines and possessing the property of causing marked panting while at the same time supporting a light. Panting is not mentioned as one of the symptoms produced by white-damp, nor has white-damp a high specific gravity.

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* "The physiological effects of Air vitiated by Respiration," 'Journal of Pathology and Bacteriology,' Oct., 1892.

Summary of Conclusions.

1. The specimens of black-damp consisted when undiluted of nitrogen containing an admixture of a seventh to an eighth of its volume of carbonic acid.

2. Air containing just sufficient black-damp to extinguish a candle or oil lamp produced no immediately sensible action on a man. A mixture of about 16 per cent. of the black-damp and 84 per cent. of air extinguished candles and lamps, whereas a mixture of about 60 per cent. of the black damp and 40 per cent. of air would be required to produce immediate danger to life.

3. The dangerous physiological action of black-damp is due to deficiency of oxygen, not to excess of carbonic acid. The effect first appreciable when increasing proportions of black-damp are breathed is, however, due to carbonic acid alone.

In conclusion, I beg to express my hearty thanks to Mr. W. N. Atkinson for his co-operation in this investigation, and to the managers of the Podmore and Lilleshall Collieries for their courtesy in doing everything possible to facilitate the work.

Note. Added January 7, 1895.

Since writing the above I have examined a specimen of black-damp from an ironstone pit (communicating with a seam of coal) at Apedale. A double analysis of the sample gave the following results:—

	No. 1.	No. 2.
Oxygen	10·07	10·09
Carbonic acid	7·64	7·60
Nitrogen	82·29	82·31
Total	100·00	100·00

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* An experiment demonstrating this fact is described by Dr. Lorrain Smith and myself. *Loc. cit.*

Calculated as above the composition of the undiluted black-damp was thus :—

Nitrogen................	85·30
Carbonic acid............	14·70

This result differs little from that given by the other samples.

II. "Mathematical Contributions to the Theory of Evolution. II. Skew Variation in Homogeneous Material." By KARL PEARSON, University College, London. Communicated by Professor HENRICI, F.R.S. Received December 19, 1894.

(Abstract.)

PART I.—*Theoretical.*

In the deduction of the normal curve of frequency from the symmetrical point binomial, three conditions are usually assumed to be true :—

(*a*.) The chances of any "contributory cause" giving its unit of deviation in excess or in defect are presumed to be equal.

(*b*.) The number of "contributory causes" are supposed to be indefinitely great.

(*c*.) The "contributory causes" are all supposed to be independent.

(*c*) amounts to the assumption of a binomial form $(p+q)^n$, (*a*) to the equality of p and q, (*c*) to the indefinitely great value of n.

It is shown in the paper that there is an important geometrical relation between the normal frequency curve

$$z = z_0 e^{-x^2/2\sigma^2},$$

and the symmetrical point binomial, $(\frac{1}{2}+\frac{1}{2})^n$, which is true independently of the magnitude of n. Thus the condition (*b*) is not necessary to the very close fitting of symmetrical point binomials to normal curves for even very small values of n, such, for example, as 8 or 10. This has been long recognised in statistical practice if its source has not been noted.

We can remove the condition (*a*) from our *à priori* limitations by finding a curve which is related to the skew binomial $(p+q)^n$ in precisely the same manner as the normal curve is related to the symmetrical binomial $(\frac{1}{2}+\frac{1}{2})^n$. The equation to this curve is

$$z = z_0 \left(1+\frac{x}{a}\right)^p e^{-\gamma x}.$$

If α be the total frequency, and $\mu_r \alpha$ the rth moment of the frequency curve about its centroid vertical, then for this curve

$$2\mu_2(3\mu_2^2 - \mu_4) + 3\mu_3^2 = 0.$$

This relation must be satisfied or nearly satisfied if a series of observations or measurements is to be fitted with the skew curve, which is related to the skew point-binomial as the normal curve to the symmetrical point-binomial. For fitting a skew point-binomial we must have

$$\mu_4 < 3\mu_2^2 + 3\mu_3^2/(2\mu_2).$$

For the normal curve $\mu_4 = 3\mu_2^2$. But a great number of statistical returns—especially in anthropometry and zoometry—give

$$\mu_4 > 3\mu_2^2 + 3\mu_3^2/(2\mu_2).$$

Hence they differ from the normal curve in the opposite direction to the skew point-binomial and its corresponding frequency curve.

After the complete theory of the fitting of skew binomials and this special skew curve has been discussed with examples, the memoir proceeds to the generalisation of the frequency curve by withdrawing the limitation (*c*) above. Just as the symmetrical binomial and normal curves are illustrated by the tossing of a group of n coins, and the skew binomial and its skew curve by the spinning of a group of n m-sided teetotums, so we can arrive at a series of curves in which the contributory causes are interdependent, by considering the withdrawal of r cards from a pack of ns cards containing s suits; or, again, by drawing a definite amount of sand from a vessel containing two kinds of sand.

For discontinuous series the solution is a hypergeometrical series. If now a curve be formed which is related by the same fundamental geometrical relation to this hypergeometrical series as the normal curve to the symmetrical point-binomial, or the first skew curve to the skew point-binomial, we obtain a generalised frequency curve which contains both those hitherto considered as special or limiting cases.

It is not suggested that the hypergeometrical series or its corresponding curve is the only case in which the *à priori* condition (*c*) of dependence of "contributory causes" is replaced by an interdependence. But it is suggested that it is one of the most important cases, and one which naturally occurs at the commencement of our investigations. That it is probably quite sufficient is evidenced by the fact that the author has hitherto failed to find any group of homogeneous and skew statistics which cannot be closely expressed by the curves which correspond to the hypergeometrical series.

The differential equation to the generalised frequency curve is shown to be of the form

$$\frac{1}{z}\frac{dz}{dx} = -\frac{x}{\beta_1+\beta_2 x+\beta_3 x^2}.$$

If we put $\beta_3 = 0$ we have the curve corresponding to the skew-binomial; if we put $\beta_2 = \beta_3 = 0$ we have the normal curve. In the most general case we are led to two principal types of curves

(i.) $$z = z_0 \frac{1}{\left(1+\left(\frac{x}{a}\right)^2\right)^m} e^{-\gamma \tan^{-1}\frac{x}{a}}.$$

(ii.) $$z = z_0\left(1+\frac{x}{a_1}\right)^{m_1}\left(1-\frac{x}{a_2}\right)^{m_2}.$$

The second of these curves is marked by a limited range and skewness. Its theory—method of fitting to actual statistics and its geometrical properties—are discussed, and the curve is shown to involve in fitting only the use of a table of Γ-functions—a table which already exists.

The first of these curves has skewness but no limit to range. This unlimitedness of range is not, however, necessarily significant. There is a limit to the height of adult males, or at any rate to the ratio of their sitting to standing height, but we do not hesitate to express the results in terms of the normal curve. The fact is that both normal curve and generalised curve are only close approximations to series—point-binomial and point-hypergeometrical series—which can themselves give a limited range, and we ought to fit these series rather than the curves to our observations.*

The criterion to distinguish between the application to any special case of curves (i) or (ii) is the negative or positive value of

$$2\mu_2(3\mu_2^2-\mu_4)+3\mu_3^2,$$

which we have seen vanishes for the curve corresponding to the skew point-binomial.

The complete treatment of curves of the first kind is shown to depend on a certain integral called a G-function. This G-function has been discussed in a recent paper by Dr. Forsyth, to whom the author had referred for information with regard to it. It is built up of Γ-functions with imaginary arguments. The function has not yet been tabulated, but various formulæ are given for its evaluation, and it is hoped that its values may shortly be calculated for the range of arguments having more special practical interest.* The theory of the whole system of skew curves and their limiting cases is then discussed.

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* The fitting of the first series is discussed in this memoir; the fitting of hypergeometrical series is reserved as the memoir is already of considerable length.

The author regrets that while he has obtained a criterion for each species of skew curve, he has hitherto failed to find one which will distinguish a compound curve, *e.g.*, heterogeneous material, from a skew curve resulting from skew variation in homogeneous material. He does not despair, however, of ultimately finding such a criterion. The test of actual fitting is generally sufficient, but is, of course, laborious.†

PART II.—*Illustration.*

The second part of the memoir provides the minimum of illustration, which the author considers absolutely necessary, in order to demonstrate that the generalised curves reached are capable of the widest application to every variety of practical statistics. The illustrations show that from the slight amount of skewness usually neglected by statisticians—although of vital import when we come to consider variation with growth, as in statistics of child-variation with growth—even to extreme cases in which the curve is asymptotic to the ordinate of maximum frequency, a good fitting generalised frequency curve can be found. Although the number of illustrations is considerable, and is only a part of the author's material, yet he hesitates at present to make any dogmatic statements with regard to the relations of skewness in variation to secular evolution; but he believes that the persistent recurrence of certain types of curves in zoometry and of certain directions of skewness in anthropometric statistics will be found, as sufficient material accumulates, to justify broad generalisations, although at present they can only be treated as suggestions for further investigation.

The special illustrations given are: barometer variation (Venn), variation in crabs and prawns (Weldon), in height of American recruits (Baxter), American school girls (Porter), in length-breadth index of Bavarian skulls (Ranke), frequency of enteric fever (Metropolitan Asylums Board), guesses at tints (Gresham College), divorce statistics (Willcox), variation in house-value (Goschen), variation in buttercups and clover (De Vries), variation in pauper percentages (Booth), and resolution of the English male mortality curve (Ogle) into skew components.

The memoir concludes with some general remarks on the modifications required in the theory of correlation by the use of generalised curves, but reserves for the present its complete discussion.

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* The British Association have kindly given a grant for this purpose.

† It is noteworthy that all cases of compoundedness dealt with hitherto by the author give $2\mu_2(3\mu_2^2-\mu_4)+3\mu_3^2$ positive.

III. "On the Distribution of Assimilated Iron Compounds, other than Hæmoglobin and Hæmatins, in Animal and Vegetable Cells. Preliminary Communication." By A. B. MACALLUM, Associate-Professor of Physiology, University of Toronto. Communicated by Professor M. FOSTER, Sec. R.S. Received December 3, 1894.

The question of the presence of iron in the chromatin of animal and vegetable cells and the methods of demonstrating the element in this substance were discussed in a communication to the Royal Society made more than three years ago.* Since then I have endeavoured to determine with perfected methods the distribution of assimilated iron compounds in cells of all classes, and have succeeded in obtaining results of which the present communication is an exceedingly condensed and preliminary account. A full account will be published elsewhere.

The methods adopted were such as prevented a confusion of the iron of inorganic and albuminate combinations with that of assimilated compounds. The reagent which proved to be of the greatest service was freshly prepared ammonium hydrogen sulphide made from a solution of ammonia of 0·96 specific gravity, and applied, mixed with glycerine, to the isolated cells in the way already described. Sulphuric, hydrochloric, and nitric acids, dissolved in certain proportions in alcohol of 95 per cent. strength, have been found to liberate the iron of assimilated compounds, but the results obtained with these acids were, in all cases where this was possible, controlled by experiments with the sulphide reagent. The iron liberated was readily demonstrated in the form of ferrous sulphide or of the Prussian-blue compound.

The fact that the iron of coagulated hæmoglobin is unaffected by ammonium hydrogen sulphide enables one to overcome the difficulties presented by the presence of that colouring matter in many animal forms. The iron of hæmatin is, however, liberated by that reagent, but the rapidity with which this is done, under the most ordinary conditions, may be employed to distinguish the iron so derived from that of other organic compounds. Whether chlorophyll contains iron as a constituent of its molecule is still a matter of dispute, but the presence of that colouring matter in vegetable cells does not complicate the results, since in the hardening processes, especially when alcohol is used, it may be wholly removed from vegetable tissues, which then, so far as the distribution of "masked" iron is concerned, give no evidence of anything different from what obtains in *Monotropa uniflora* and *Corallorhiza multiflora*—phanerogamous plants destitute of chlorophyll.

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* "On the Demonstration of the Presence of Iron in Chromatin by Micro-chemical Methods," 'Roy. Soc. Proc.,' vol. 50, p. 277.

Some of the more important facts ascertained in the investigation may be thus briefly stated:—

1. Iron, firmly combined, is a constant constituent of animal and vegetable chromatin. Another compound, less rich in iron, is found in nucleoli.

2. The chromophilous substance in ferment-forming cells contains iron, and the cytoplasm of Protozoan organisms, which also probably secretes ferments, yields evidence of the presence of a firmly combined iron compound.

3. A firm compound of iron is present in the chromophilous substance of the cytoplasm of Fungi.

4. Of the non-nucleated organisms, Bacteria, owing to their minuteness, have, with one exception, given little evidence of the presence of an organic iron compound; but in the Cyanophyceæ the chromophilous portions of the "central substance" contain iron, and iron may be also demonstrated in the peripheral granules formed of the so-called cyanophycin (Palla).

IV. "Micro-metallography of Iron. Part I." By THOMAS ANDREWS, F.R.S. Received December 15, 1894.

[Publication deferred.]

Presents, January 24, 1895.

Transactions.

Bergen:—Museum. Aarbog. 1893. 8vo. *Bergen* 1894; On the Development and Structure of the Whale. Part 1. By G. Guldberg and F. Nansen. 4to. *Bergen* 1894.
The Museum.

Berlin:—Gesellschaft für Erdkunde. Verhandlungen. Bd. XXI. No. 10. 8vo. *Berlin* 1894.
The Society.

Christiania:—Videnskabs Selskab. Forhandlinger. 1893. 8vo. *Christiania.*
The Society.

Dublin:—Royal Irish Academy. Proceedings. Third Series. Vol. III. No. 3. 8vo. *Dublin* 1894.
The Academy.

Hermannstadt:—Siebenbürgischer Verein für Naturwissenschaften. Verhandlungen und Mittheilungen. Jahrg. XLII. 8vo. *Hermannstadt* 1892.
The Society.

Johannesburg:—South African Association of Engineers and Architects. Proceedings. 1892–94. 8vo. *London.*
The Association.

Transactions (*continued*).

London :—British Museum. Catalogue of Printed Books. R—Rag. Rah—Rap. Rap—Rea. Rea—Reg. Rem—Rev. Folio. *London* 1894. The Trustees.

Institution of Mechanical Engineers. 1894. No. 3. 8vo. *London.* The Institution.

Linnean Society. Journal (Zoology). Vol. XXV. Nos. 159—160. 8vo. *London* 1894; List of Fellows. 1894-95. 8vo. *London.* The Society.

London Mathematical Society. Proceedings. Vol. XXV. Nos. 495—499. 8vo. *London* 1894; List of Members. 1894. 8vo. *London.* The Society.

Odontological Society. Transactions. Vol. XXVII. No. 2. 8vo. *London* 1894. The Society.

Physical Society. Proceedings. Vol. XIII. Part 2. 8vo. *London* 1895. The Society.

Royal Agricultural Society. Journal. Vol. V. Part 4. 8vo. *London* 1894. The Society.

Royal Horticultural Society. Journal. Vol. XVIII. 8vo. *London* 1895; Report of the Council for the year 1894-95. 8vo. *London.* The Society.

Manchester :—Literary and Philosophical Society. Memoirs and Proceedings. Vol. IX. No. 1. 8vo. *Manchester* 1895. The Society.

Milan :—Fondazione Scientifica Cagnola. Atti. Vol. XI. 8vo. *Milano* 1893. The Institution.

R. Istituto Lombardo di Scienze e Lettere. Memorie. Vol. XVII. Fasc. 2, 3. 4to. *Milano* 1892-94; Rendiconti. Vols. XXV, XXVI. 8vo. *Milano* 1892, 1893; Indice Generale dei Lavori dalla Fondazione all' Anno 1888 per Autori e per Materie. 8vo. *Milano* 1891. The Institute.

New York :—American Museum of Natural History. Bulletin. Vol. VI. Pages 353—384. 8vo. [*New York*] 1894. The Museum.

Tromsö :—Museum. Aarsberetning. 1892. 8vo. *Tromsö* 1893; Aarshefter. 16. 8vo. *Tromsö* 1894. The Museum.

Observations and Reports.

Bombay :—Meteorological Office. Brief Sketch of the Meteorology of the Bombay Presidency in 1893-94. Folio. [*Bombay.*] The Office.

Calcutta :—Meteorological Department, Government of India. Monthly Weather Review. July, August, 1894. 4to. *Cal-*

Observations and Reports (*continued*).

cutta; Meteorological Observations recorded at Seven Stations in India. July, August, 1894. 4to. [*Calcutta.*]
The Department.

International:—Observations Internationales Polaires, 1882–83. Observations faites à Godthaab. Tome I. Livr. 2. 4to. *Copenhague* 1894. The Meteorological Office, London.

London:—Medical Department of the Navy. Statistical Report of the Health of the Navy. 1893. 8vo. *London* 1894.
The Department.

Nautical Almanac Office. The Nautical Almanac and Astronomical Ephemeris for the year 1898. 8vo. *London.*
The Admiralty.

Washington:—Weather Bureau. Report of the Ohio Weather and Crop Service. November, 1894. 8vo. *Columbus, Ohio.*
The Bureau.

Journals.

Astronomy and Astro-Physics. October—December, 1894. 8vo. *Northfield, Minn.* The Editors.

Fortschritte der Physik. 1888. 8vo. *Braunschweig* 1894
Physikalische Gesellschaft, Berlin.

Horological Journal. Vol. XXXVII. No. 437. 8vo. *London* 1895. British Horological Institute.

Journal of Comparative Neurology. December, 1894. 8vo. *Granville, Ohio.* The Editor.

Sbornik Materialov dlya Opisaniya Myestenosteii i Plemen Kavkaza. Vol. XIX. 8vo. *Tiflis* 1894.
Curateur de l'Arrondissement Scolaire du Caucase.

Technology Quarterly. Vol. VII. No. 2. 8vo. *Boston* 1894.
Massachusetts Inst. of Technology.

Denza (F.) Cenni Necrologici. 8vo. *Roma* 1894.
Rev. P. Armani.

Helmholtz (H. von) Handbuch der Physiologischen Optik. 2te Auflage. Lief. 9. 8vo. *Hamburg* 1894. The Publisher.

Hunt (H. A.) An Essay on Southerly Bursters. 8vo. [*Sydney* 1894.] Mr. H. E. Hunt.

Mourlon (M.) Le Service de la Carte Géologique et les Conséquences de son Organisation. 8vo. *Bruxelles* 1894.
The Author.

Riefler (S.) Die Präcisions-Uhren mit vollkommen freiem Echappement und neuem Quecksilber-Compensationspendel. 8vo. *München* 1894. The Author.

Sidgreaves (Rev. W.) Notes on Solar Observations at Stonyhurst College Observatory. 8vo. *London* 1894. The Author.

Walker (G. C.) . Customary Law of the Main Tribes in the Lahore District. Vol. XIII. 8vo. *Lahore* 1894. India Office.

Map showing Lines of Equal Magnetic Declination for January 1, 1895. With a Reprint from the 'Colliery Guardian.'
The Editor, 'Colliery Guardian.'

January 31, 1895.

(In the Theatre of the University of London.)

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Argon, a New Constituent of the Atmosphere." By LORD RAYLEIGH, Sec. R.S., and WILLIAM RAMSAY, F.R.S., Professor of Chemistry, University College, London. Received January 31, 1895.

(Abstract.)

I. *Density of Nitrogen from Various Sources.*

In a former paper[*] it has been shown that nitrogen extracted from chemical compounds is about $\frac{1}{2}$ per cent. lighter than "atmospheric nitrogen."

The mean numbers for the weights of gas contained in the globe used were as follows:—

	Grams.
From nitric oxide	2·3001
From nitrous oxide............	2·2990
From ammonium nitrite	2·2987

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[*] Rayleigh, "On an Anomaly encountered in Determinations of the Density of Nitrogen Gas." 'Roy. Soc. Proc.,' vol. 55, p. 340, 1894.

while for "atmospheric nitrogen" there was found—

By hot copper, 1892	2·3103
By hot iron, 1893	2·3100
By ferrous hydrate, 1894.......	2·3102

At the suggestion of Professor Thorpe experiments were subsequently tried with nitrogen liberated from *urea* by the action of sodium hypobromite. The hypobromite was prepared from commercial materials in the proportions recommended for the analysis of urea. The reaction was well under control, and the gas could be liberated as slowly as desired.

In the first experiment the gas was submitted to no other treatment than slow passage through potash and phosphoric anhydride, but it soon became apparent that the nitrogen was contaminated. The "inert and inodorous" gas attacked vigorously the mercury of the Töpler pump, and was described as smelling like a dead rat. As to the weight, it proved to be in excess even of the weight of atmospheric nitrogen.

The corrosion of the mercury and the evil smell were in great degree obviated by passing the gas over hot metals. For the fillings of June 6, 9, and 13 the gas passed through a short length of tube containing copper in the form of fine wire heated by a flat Bunsen burner, then through the furnace over red-hot iron, and back over copper oxide. On June 19 the furnace tubes were omitted, the gas being treated with the red-hot copper only. The mean result, reduced so as to correspond with those above quoted, is 2·2985.

Without using heat, it has not been found possible to prevent the corrosion of the mercury. Even when no urea is employed, and air simply bubbled through the hypobromite solution is allowed to pass with constant shaking over mercury contained in a U tube, the surface of the metal was soon fouled.

Although the results relating to urea nitrogen are interesting for comparison with that obtained from other nitrogen compounds, the original object was not attained on account of the necessity of retaining the treatment with hot metals. We have found, however, that nitrogen from ammonium nitrite may be prepared without the employment of hot tubes, whose weight agrees with that above quoted. It is true that the gas smells slightly of ammonia, easily removable by sulphuric acid, and apparently also of oxides of nitrogen. The mean result from three fillings is 2·2987.

It will be seen that, in spite of the slight nitrous smell, there is no appreciable difference in the densities of gas prepared from ammonium nitrite with and without the treatment by hot metals. The result is interesting as showing that the agreement of numbers obtained for chemical nitrogen does not depend upon the use of a red heat in the process of purification.

The five results obtained in more or less distinct ways for chemical nitrogen stand thus:—

From nitric oxide	2·3001
From nitrous oxide.........................	2·2990
From ammonium nitrite purified at a red heat	2·2987
From urea	2·2985
From ammonium nitrite purified in the cold ..	2·2987
Mean..............	2·2990

These numbers, as well as those above quoted for "atmospheric nitrogen," are subject to a deduction of 0·0006 for the shrinkage of the globe when exhausted.* If they are then multiplied in the ratio of 2·3108 : 1·2572, they will express the weights of the gas in grams per litre. Thus, as regards the mean numbers, we find as the weight per litre under standard conditions of chemical nitrogen 1·2505, that of atmospheric nitrogen being 1·2572.

It is of interest to compare the density of nitrogen obtained from chemical compounds with that of oxygen. We have $N_2 : O_2 = 2{\cdot}2984 : 2{\cdot}6276 = 0{\cdot}87471$; so that if $O_2 = 16$, $N_2 = 13{\cdot}9954$. Thus, when the comparison is with chemical nitrogen, the ratio is very nearly that of 16 : 14; but if "atmospheric nitrogen" be substituted, the ratio of small integers is widely departed from.

To the above list may be added nitrogen prepared in yet another manner, whose weight has been determined subsequently to the isolation of the new dense constituent of the atmosphere. In this case nitrogen was actually extracted from air by means of magnesium. The nitrogen thus separated was then converted into ammonia by action of water upon the magnesium nitride and afterwards liberated in the free state by means of calcium hypochlorite. The purification was conducted in the usual way, and included passage over red-hot copper and copper oxide. The following was the result:—

Globe empty, Oct. 30, Nov. 5..........	2·82313
Globe full, Oct. 31	0·52395
Weight of gas	2·29918

It differs inappreciably from the mean of other results, viz., 2·2990, and is of special interest as relating to gas which at one stage of its history formed part of the atmosphere.

Another determination, with a different apparatus, of the density of "chemical" nitrogen from the same source, magnesium nitride, which had been prepared by passing "atmospheric" nitrogen over ignited magnesium, may here be recorded. The sample differed from that previously mentioned, inasmuch as it had not been subjected to treatment with red-hot copper. After treating the nitride with water, the resulting ammonia was distilled off, and collected in hydrochloric acid; the solution was evaporated by degrees, the dry ammonium chloride was dissolved in water, and its concentrated solution added to a freshly-prepared solution of sodium hypobromite. The nitrogen was collected in a gas-holder over water which had previously been boiled, so as, at all events partially, to expel air. The nitrogen passed into the vacuous globe through a solution of potassium hydroxide, and through two drying-tubes, one containing soda-lime, and the other phosphoric anhydride.

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* Rayleigh, "On the Densities of the Principal Gases," 'Roy. Soc. Proc.,' vol. 53, p. 134, 1893.

At 18·38° C. and 754·4 mm. pressure, 162·843 c.c. of this nitrogen weighed 0·18963 gram. Hence,

Weight of 1 litre at 0° C. and 760 mm. pressure = 1·2521 gram.

The mean result of the weight of 1 litre of "chemical" nitrogen has been found to equal 1·2505. It is therefore seen that "chemical" nitrogen, derived from "atmospheric" nitrogen, without any exposure to red-hot copper, possesses about the usual density.

Experiments were also made, which had for their object to prove that the ammonia produced from the magnesium nitride is identical with ordinary ammonia, and contains no other compound of a basic character. For this purpose the ammonia was converted into ammonium chloride, and the percentage of chlorine determined by titration with a solution of silver nitrate which had been standardised by titrating a specimen of pure sublimed ammonium chloride. The silver solution was of such a strength that 1 c.c. precipitated the chlorine from 0·001701 gram of ammonium chloride.

1. Ammonium chloride from orange-coloured sample of magnesium nitride contained 66·35 per cent. of chlorine.

2. Ammonium chloride from blackish magnesium nitride contained 66·35 per cent. of chlorine.

3. Ammonium chloride from nitride containing a large amount of unattacked magnesium contained 66·30 per cent. of chlorine.

Taking for the atomic weights of hydrogen H = 1·0032, of nitrogen N = 14·04, and of chlorine Cl = 35·46, the theoretical amount of chlorine in ammonium chloride is 66·27 per cent.

From these results—that nitrogen prepared from magnesium nitride, obtained by passing "atmospheric" nitrogen over red-hot magnesium has the density of "chemical" nitrogen, and that ammonium chloride, prepared from magnesium nitride, contains practically the same percentage of chlorine as pure ammonium chloride—it may be concluded that red-hot magnesium withdraws from "atmospheric nitrogen" no substance other than nitrogen capable of forming a basic compound with hydrogen.

II. *Reasons for suspecting a hitherto Undiscovered Constituent in Air.*

When the discrepancy of weights was first encountered, attempts were naturally made to explain it by contamination with known impurities. Of these the most likely appeared to be hydrogen, present in the lighter gas in spite of the passage over red-hot cupric oxide. But inasmuch as the intentional introduction of hydrogen into the heavier gas, afterwards treated in the same way with cupric oxide, had no effect upon its weight, this explanation had to be abandoned, and finally it became clear that the difference could not be accounted for by the presence of any known impurity. At this stage it seemed not improbable that the lightness of the gas extracted from chemical compounds was to be explained by partial dissociation of nitrogen molecules N_2 into detached atoms. In order to test this suggestion both kinds of gas were submitted to the action of the silent electric discharge, with the result that both retained their weights unaltered. This was discouraging, and a further experiment pointed still more markedly in the negative direction. The chemical behaviour of nitrogen is such as to suggest that dissociated atoms would possess a high degree of activity, and that even though they might be formed in the first instance their life would probably be short. On standing they might be expected to disappear, in partial analogy with the known behaviour of ozone. With this idea in view, a sample of chemically prepared nitrogen was stored for eight months. But at the end of this time the density showed no sign of increase, remaining exactly as at first.*

Regarding it as established that one or other of the gases must be a mixture, containing, as the case might be, an ingredient much heavier or much lighter than ordinary nitrogen, we had to consider the relative probabilities of the various possible interpretations. Except upon the already discredited hypothesis of dissociation, it was difficult to see how the gas of chemical origin could be a mixture. To suppose this would be to admit two kinds of nitric acid, hardly reconcilable with the work of Stas and others upon the atomic weight of that substance. The simplest explanation in many respects was to admit the existence of a second ingredient in air from which oxygen, moisture, and carbonic anhydride had already been removed. The proportional amount required was not great. If the density of the supposed gas were double that of nitrogen, ½ per cent. only by volume would be needed; or if the density were but half as much again as that of nitrogen, then 1 per cent. would still suffice. But in accepting this explanation, even provisionally, we had to face the improbability that a gas surrounding us on all sides, and present in enormous quantities, could have remained so long unsuspected.

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* 'Roy. Soc. Proc.,' vol. 55, p. 344, 1894.

The method of most universal application by which to test whether a gas is pure or a mixture of components of different densities is that of diffusion. By this means Graham succeeded in effecting a partial separation of the nitrogen and oxygen of the air, in spite of the comparatively small difference of densities. If the atmosphere contain an unknown gas of anything like the density supposed, it should be possible to prove the fact by operations conducted upon air which had undergone atmolysis. This experiment, although in view from the first, was not executed until a later stage of the inquiry (§ VI), when results were obtained sufficient of themselves to prove that the atmosphere contains a previously unknown gas.

But although the method of diffusion was capable of deciding the main, or at any rate the first question, it held out no prospect of isolating the new constituent of the atmosphere, and we, therefore, turned our attention in the first instance to the consideration of methods more strictly chemical. And here the question forced itself upon us as to what really was the evidence in favour of the prevalent doctrine that the inert residue from air after withdrawal of oxygen, water, and carbonic anhydride, is all of one kind.

The identification of "phlogisticated air" with the constituent of nitric acid is due to Cavendish, whose method consisted in operating with electric sparks upon a short column of gas confined with potash over mercury at the upper end of an inverted U tube.*

Attempts to repeat Cavendish's experiment in Cavendish's manner have only increased the admiration with which we regard this wonderful investigation. Working on almost microscopical quantities of material, and by operations extending over days and weeks, he thus established one of the most important facts in chemistry. And what is still more to the purpose, he raises as distinctly as we could do, and to a certain extent resolves, the question above suggested. The passage is so important that it will be desirable to quote it at full length.

"As far as the experiments hitherto published extend, we scarcely know more of the phlogisticated part of our atmosphere, than that it is not diminished by lime-water, caustic alkalies, or nitrous air; that it is unfit to support fire, or maintain life in animals; and that its specific gravity is not much less than that of common air: so that though the nitrous acid, by being united to phlogiston,† is converted into air possessed of these properties, and consequently, though it was reasonable to suppose, that part at least of the phlogisticated air of the atmosphere consists of this acid united to phlogiston, yet it was fairly to be doubted whether the whole is of this kind, or whether there are not in reality many different substances compounded together by us under the name of phlogisticated air. I therefore made an experiment to determine whether the whole of a given portion of the phlogisticated air of the atmosphere could be reduced to nitrous acid, or whether there was not a part of a different nature to the rest, which would refuse to undergo that change. The foregoing experiments indeed in some measure decided this point, as much the greatest part of the air let up into the tube lost its elasticity; yet as some remained unabsorbed it did not appear for certain whether that was of the same nature as the rest or not. For this purpose I diminished a similar mixture of dephlogisticated and common air, in the same manner as before, till it was reduced to a small part of its original bulk. I then, in order to decompound as much as I could of the phlogisticated air which remained in the tube, added some dephlogisticated air to it, and continued the spark until no further diminution took place. Having by these means condensed as much as I could of the phlogisticated air, I let up some solution of liver of sulphur to absorb the dephlogisticated air; after which only a small bubble of air remained unabsorbed, which certainly was not more than $\frac{1}{120}$ of the bulk of the phlogisticated air let up into the tube; so that if there is any part of the phlogisticated air of our atmosphere which differs from the rest, and cannot be reduced to nitrous acid, we may safely conclude that it is not more than $\frac{1}{120}$th part of the whole."

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* "Experiments on Air," 'Phil. Trans.,' vol. 75, p. 372, 1785.

† *I.e.*, deprived of oxygen. "Phlogisticated air" = nitrogen, "dephlogisticated air" = oxygen.

Although Cavendish was satisfied with his result, and does not decide whether the small residue was genuine, our experiments about to be related render it not improbable that his residue was really of a different kind from the main bulk of the "phlogisticated air," and contained the gas now called argon.

Cavendish gives data[*] from which it is possible to determine the rate of absorption of the mixed gases in his experiment. This was about 1 c.c. per hour, of which two-fifths would be nitrogen.

III. *Methods of Causing Free Nitrogen to Combine.*

To eliminate nitrogen from air, in order to ascertain whether any other gas could be detected, involves the use of some absorbent. The elements which have been found to combine directly with nitrogen are: boron, silicon, titanium, lithium, strontium, barium, magnesium, aluminium, mercury, and, under the influence of an electric discharge, hydrogen in presence of acid, and oxygen in presence of alkali.

[*] 'Phil. Trans.,' vol. 78, p. 271, 1788.

Besides these, a mixture of barium carbonate and carbon at a high temperature is known to be effective. Of those tried, magnesium in the form of turnings was found to be the best. When nitrogen is passed over magnesium, heated in a tube of hard glass to bright redness, combustion with incandescence begins at the end of the tube through which the gas is introduced, and proceeds regularly until all the metal has been converted into nitride. Between 7 and 8 litres of nitrogen can be absorbed in a single tube; the nitride formed is a porous, dirty orange-coloured substance.

IV. *Early Experiments on Sparking Nitrogen with Oxygen in presence of Alkali.*

In our earliest attempts to isolate the suspected gas by the method of Cavendish, we used a Ruhmkorff coil of medium size actuated by a battery of five Grove cells. The gases were contained in a test-tube standing over a large quantity of weak alkali, and the current was conveyed in wires insulated by U-shaped glass tubes passing through the liquid round the mouth of the test-tube. With the given battery and coil a somewhat short spark or arc of about 5 mm. was found to be more favourable than a longer one. When the mixed gases were in the right proportion the rate of absorption was about 30 c.c. per hour, or thirty times as fast as Cavendish could work with the electrical machine of his day.

To take an example, one experiment of this kind started with 50 c.c. of air. To this oxygen was gradually added until, oxygen being in excess, there was no perceptible contraction during an hour's sparking. The remaining gas was then transferred at the pneumatic trough to a small measuring vessel, sealed by mercury, in which the volume was found to be 1·0 c.c. On treatment with alkaline pyrogallate, the gas shrank to 0·32 c.c. That this small residue could not be nitrogen was argued from the fact that it had withstood the prolonged action of the spark, although mixed with oxygen in nearly the most favourable proportion.

The residue was then transferred to the test-tube with an addition of another 50 c.c. of air, and the whole worked up with oxygen as before. The residue was now 2·2 c.c., and, after removal of oxygen, 0·76 c.c.

Although it seemed almost impossible that these residues could be either nitrogen or hydrogen, some anxiety was not unnatural, seeing that the final sparking took place under somewhat abnormal conditions. The space was very restricted, and the temperature (and with it the proportion of aqueous vapour) was unduly high. But any doubts that were felt upon this score were removed by comparison experiments in which the whole quantity of air operated on was very small. Thus, when a mixture of 5 c.c. of air with 7 c.c. of oxygen was sparked for 1¼ hours, the residue was 0·47 c.c., and after removal of oxygen 0·06 c.c. Several repetitions having given similar results, it became clear that the final residue did not depend upon anything that might happen when sparks passed through a greatly reduced volume, *but was in proportion to the amount of air operated upon.*

No satisfactory examination of the residue which refused to be oxidised could be made without the accumulation of a larger quantity. This, however, was difficult of attainment at the time in question. It was thought that the cause probably lay in the solubility of the gas in water, a suspicion since confirmed. At length, however, a sufficiency was collected to allow of sparking in a specially constructed tube, when a comparison with the air spectrum, taken under similar conditions, proved that, at any rate, the gas was not nitrogen. At first scarcely a trace of the principal nitrogen lines could be seen, but after standing over water for an hour or two these lines became apparent.

V. *Early Experiments on Withdrawal of Nitrogen from Air by means of Red-hot Magnesium.*

A preliminary experiment carried out by Mr. Percy Williams on the absorption of atmospheric nitrogen, freed from oxygen by means of red-hot copper, in which the gas was not passed over, but simply allowed to remain in contact with the metal, gave a residue of density 14·88. This result, although not conclusive, was encouraging; and an attempt was made, on a larger scale, by passing atmospheric nitrogen backwards and forwards over red-hot magnesium from one large gas-holder to another to obtain a considerable quantity of the heavier gas. In the course of ten days, about 1500 c.c. were collected and transferred gradually to a mercury gas-holder, from which the gas was passed over soda-lime, phosphoric anhydride, magnesium at a red heat, copper oxide, soda-lime, and phosphoric anhydride into a second mercury gas-holder. After some days the gas was reduced in volume to about 200 c.c., and its density was found to be 16·1. After further absorption, in which the volume was still further reduced, the density of the residue was increased to 19·09.

On passing sparks for several hours through a mixture of a small quantity of this gas with oxygen, its volume was still further reduced. Assuming that this reduction was due to the further elimination of nitrogen, the density of the remaining gas was calculated to be 20·0.

The spectrum of the gas of density 19·09, though showing nitrogen bands, showed many other lines which were not recognisable as belonging to any known element.

VI. *Proof of the Presence of Argon in Air by means of Atmolysis.*

It has already (§ II) been suggested that if "atmospheric nitrogen" contains two gases of different densities, it should be possible to obtain direct evidence of the fact by the method of atmolysis. The present section contains an account of carefully conducted experiments directed to this end.

The atmolyser was prepared (after Graham) by combining a number of "churchwarden" tobacco pipes. At first twelve pipes were used in three groups, each group including four pipes connected in series. The three groups were then connected in parallel, and placed in a large glass tube closed in such a way that a partial vacuum could be maintained in the space outside the pipes by a water pump. One end of the combination of pipes was open to the atmosphere; the other end was connected to a bottle aspirator, initially full of water, and so arranged as to draw about 2 per cent. of the air which entered the other end of the pipes. The gas collected was thus a very small proportion of that which leaked through the pores of the pipes, and should be relatively rich in the heavier constituents of the atmosphere. The flow of water from the aspirator could not be maintained very constant, but the rate of 2 per cent. was never much exceeded.

The air thus obtained was treated exactly as ordinary air had been treated in determinations of the density of atmospheric nitrogen. Oxygen was removed by red-hot copper, followed by cupric oxide, ammonia by sulphuric acid, moisture and carbonic acid by potash and phosphoric anhydride.

In a total weight of approximately 2·3 grams the excess of weight of the diffused nitrogen over ordinary atmospheric nitrogen was in four experiments, 0·0049, 0·0014, 0·0027, 0·0015.

The mean excess of the four determinations is 0·00262 gram, or, if we omit the first, which depended upon a vacuum weighing of two months old, 0·00187 gram.

The gas from prepared air was thus in every case denser than from unprepared air, and to an extent much beyond the possible errors of experiment. The excess was, however, less than had been expected, and it was thought that the arrangement of the pipes could be improved. The final delivery of gas from each of the groups in parallel being so small in comparison with the whole streams concerned, it seemed possible that each group was not contributing its proper share, and even that there might be a flow in the wrong direction at the delivery end of one or two of them. To meet this objection, the arrangement in parallel had to be abandoned, and for the remaining experiments eight pipes were connected in simple series. The porous surface in operation was thus reduced, but this was partly compensated for by an improved vacuum. Two experiments were made under the new conditions, in which the excess was I, 0·0037; II, 0·0033.

The excess being larger than before is doubtless due to the greater efficiency of the atmolysing apparatus. It should be mentioned that the above recorded experiments include all that have been tried, and the conclusion seems inevitable that "atmospheric nitrogen" is a mixture, and not a simple body.

It was hoped that the concentration of the heavier constituent would be sufficient to facilitate its preparation in a pure state by the use of prepared air in substitution for ordinary air in the oxygen apparatus. The advance of 3½ milligrams on the 11 milligrams, by which atmospheric nitrogen is heavier than chemical nitrogen, is indeed not to be despised, and the use of prepared air would be convenient if the diffusion apparatus could be set up on a large scale and be made thoroughly self-acting.

VII. *Negative Experiments to prove that Argon is not derived from Nitrogen from Chemical Sources.*

Although the evidence of the existence of argon in the atmosphere, derived from the comparison of densities of atmospheric and chemical nitrogen and from the diffusion experiments (§ VI), appeared overwhelming, we have thought it undesirable to shrink from any labour that would tend to complete the verification. With this object in view, an experiment was undertaken and carried to a conclusion on November 13, in which 3 litres of chemical nitrogen, prepared from ammonium nitrite, were treated with oxygen in precisely the manner in which atmospheric nitrogen had been found to yield a residue of argon. The gas remaining at the close of the large scale operations was worked up as usual with battery and coil until the spectrum showed only slight traces of the nitrogen lines. When cold, the residue measured 4 c.c. This was transferred, and after treatment with alkaline pyrogallate to remove oxygen measured 3·3 c.c. If atmospheric nitrogen had been employed, the final residue should have been about 30 c.c. Of the 3·3 c.c. actually left, a part is accounted for by an accident, and the result of the experiment is to show that argon is not formed by sparking a mixture of oxygen and chemical nitrogen.

In a second experiment of the same kind 5660 c.c. of nitrogen from ammonium nitrite was treated with oxygen. The final residue was 3·5 c.c., and was found to consist mainly of argon.

The source of the residual argon is to be sought in the water used for the manipulation of the large quantities of gas (6 litres of nitrogen and 11 litres of oxygen) employed. When carbonic acid was collected in a similar manner and subsequently absorbed by potash, it was found to have acquired a contamination consistent with this explanation.

Negative experiments were also carried out, absorbing nitrogen by means of magnesium. In one instance 3 litres of nitrogen prepared from ammonium chloride and bleaching-powder was reduced in volume to 4·5 c.c., and on sparking with oxygen its volume was further reduced to about 3 c.c. The residue appeared to consist of argon. Another experiment, in which 15 litres of nitrogen from ammonium nitrite was absorbed, gave a final residue of 3·5 c.c. Atmospheric nitrogen, in the latter case, would have yielded 150 c.c., hence less than $\frac{1}{40}$th of the normal quantity was obtained. It should be mentioned that leakage occurred at one stage, by which perhaps 200 c.c. of air entered the apparatus; and, besides, the nitrogen was collected over water from which it doubtless acquired some argon. Quantitative negative experiments of this nature are exceedingly difficult, and require a long time to carry them to a successful conclusion.

VIII. *Separation of Argon on a Large Scale.*

To prepare argon on a large scale, air is freed from oxygen by means of red-hot copper. The residue is then passed from a gas-holder through a combustion-tube, heated in a furnace, and containing copper, in order to remove all traces of oxygen; the issuing gas is then dried by passage over soda-lime and phosphorus pentoxide, after passage through a small U tube containing sulphuric acid, to indicate the rate of flow. It then enters a combustion-tube packed tightly with magnesium turnings, and heated to redness in a second furnace. From this tube it passes through a second index-tube, and enters a small gas-holder capable of containing 3 or 4 litres. A single tube of magnesium will absorb from 7 to 8 litres of nitrogen. The temperature must be nearly that of the fusion of the glass, and the current of gas must be carefully regulated, else the heat developed by the union of the magnesium with nitrogen will fuse the tube.

Having collected the residue from 100 or 150 litres of atmospheric nitrogen, which may amount to 4 or 5 litres, it is transferred to a small gas-holder connected with an apparatus, whereby, by means of a species of a self-acting Sprengel's pump, the gas is caused to circulate through a tube half filled with copper and half with copper oxide; it then traverses a tube half filled with soda-lime and half with phosphorus pentoxide; it then passes a reservoir of about 300 c.c. capacity, from which, by raising a mercury reservoir, it can be expelled into a small gas-holder. Next it passes through a tube containing magnesium turnings heated to bright redness. The gas is thus freed from any possible contamination with oxygen, hydrogen, or hydrocarbons, and nitrogen is gradually absorbed. As the amount of gas in the tubes and reservoir diminishes in volume, it draws supplies from the gas-holder, and, finally, the circulating system is full of argon in a pure state. The circulating system of tubes is connected with a mercury pump, so that, in changing the magnesium tube, no gas may be lost. Before ceasing to heat the magnesium tube the system is pumped empty, and the collected gas is restored to the gas-holder; finally, all the argon is transferred from the mercury reservoir to the second small gas-holder, which should preferably be filled with water saturated with argon, so as to prevent contamination from oxygen or nitrogen; or, if preferred, a mercury gas-holder may be employed. The complete removal of nitrogen from argon is very slow towards the end, but circulation for a couple of days usually effects it.

The principal objection to the oxygen method of isolating argon, as hitherto described, is the extreme slowness of the operation. In extending the scale we had the great advantage of the advice of Mr. Crookes, who not long since called attention to the flame rising from platinum terminals, which convey a high tension alternating electric discharge, and pointed out its dependence upon combustion of the nitrogen and oxygen of the air.* The plant consists of a De Meritens alternator, actuated by a gas engine, and the currents are tranformed to a high potential by means of a Ruhmkorff or other suitable induction coil. The highest rate of absorption of the mixed gases yet attained is 3 litres per hour, about 3000 times that of Cavendish. It is necessary to keep the apparatus cool, and from this and other causes a good many difficulties have been encountered.

In one experiment of this kind, the total air led in after seven days' working, amounted to 7925 c.c., and of oxygen (prepared from chlorate of potash), 9137 c.c. On the eighth and ninth days oxygen alone was added, of which about 500 c.c. was consumed, while there remained about 700 c.c. in the flask. Hence the proportion in which the air and oxygen combined was as 79 : 96. The progress of the removal of the nitrogen was examined from time to time with the spectroscope, and became ultimately very slow. At last the yellow line disappeared, the contraction having apparently stopped for two hours. It is worthy of notice that with the removal of the nitrogen, the arc discharge changes greatly in appearance, becoming narrower and blue rather than greenish in colour.

The final treatment of the residual 700 c.c. of gas was on the model of the small scale operations already described. Oxygen or hydrogen could be supplied at pleasure from an electrolytic apparatus, but in no way could the volume be reduced below 65 c.c. This residue refused oxidation, and showed no trace of the yellow line of nitrogen, even under favourable conditions.

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* 'Chemical News,' vol. 65, p. 301, 1892.

When the gas stood for some days over water, the nitrogen line reasserted itself in the spectrum, and many hours' sparking with a little oxygen was required again to get rid of it. Intentional additions of air to gas free from nitrogen showed that about 1½ per cent. was clearly, and about 3 per cent. was conspicuously, visible. About the same numbers apply to the visibility of nitrogen in oxygen when sparked under these conditions, that is, at atmospheric pressure, and with a jar connected to the secondary terminals.

IX. *Density of Argon prepared by means of Oxygen.*

A first estimate of the density of argon prepared by the oxygen method was founded upon the data already recorded respecting the volume present in air, on the assumption that the accurately known densities of atmospheric and of chemical nitrogen differ on account of the presence of argon in the former, and that during the treatment with oxygen nothing is oxidised except nitrogen. Thus, if

D = density of chemical nitrogen,
D' = " atmospheric nitrogen,
d = " argon,
α = proportional volume of argon in atmospheric nitrogen,

the law of mixtures gives

$$\alpha d + (1-\alpha) D = D',$$

or

$$d = D + (D' - D)/\alpha.$$

In this formula $D' - D$ and α are both small, but they are known with fair accuracy. From the data already given

$$\alpha = \frac{65}{0{\cdot}79 \times 7925},$$

whence if (on an arbitrary scale of reckoning) $D = 2{\cdot}2990$, $D' = 2{\cdot}3102$, we find $d = 3{\cdot}378$. Thus if N_2 be 14, or O_2 be 16, the density of argon is 20·6.

A direct determination by weighing is desirable, but hitherto it has not been feasible to collect by this means sufficient to fill the large globe employed for other gases. A *mixture* of about 400 c.c. of argon with pure oxygen, however, gave the weight 2·7315, 0·1045 in excess of the weight of oxygen, viz., 2·6270. Thus, if α be the ratio of the volume of argon to the whole volume, the number for argon will be

$$2{\cdot}6270 + 0{\cdot}1045/\alpha.$$

The value of a, being involved only in the excess of weight above that of oxygen, does not require to be known very accurately. Sufficiently concordant analyses by two methods gave $a = 0·1845$; whence for the weight of the gas we get 3·193, so that, if $O_2 = 16$, the density of the gas would be 19·45. An allowance for residual nitrogen, still visible in the gas before admixture of oxygen, raises this number to 19·7, which may be taken as the density of pure argon resulting from this determination.

X. *Density of Argon prepared by means of Magnesium.*

The density of the original sample of argon prepared has already been mentioned. It was 19·09; and, after sparking with oxygen, it was calculated to be 20·0. The most reliable results of a number of determinations give it as 19·90. The difficulty in accurately determining the density is to make sure that all nitrogen has been removed. The sample of density 19·90 showed no spectrum of nitrogen when examined in a vacuum tube. It is right, however, to remark that the highest density registered was 20·38. But there is some reason here to distrust the weighing of the vacuous globe.

XI. *Spectrum of Argon.*

The spectrum of argon, seen in a vacuum tube of about 3 mm. pressure, consists of a great number of lines, distributed over almost the whole visible field. Two lines are specially characteristic; they are less refrangible than the red lines of hydrogen or lithium, and serve well to identify the gas, when examined in this way. Mr. Crookes, who will give a full account of the spectrum in a separate communication, has kindly furnished us with the accurate wave-lengths of these lines, as well as of some others next to be described; they are respectively 696·56 and 705·64, 10^{-6} mm.

Besides these red lines, a bright yellow line, more refrangible than the sodium line, occurs at 603·84. A group of five bright green lines occurs next, besides a number of less intensity. Of the group of five, the second, which is perhaps the most brilliant, has the wave-length 561·00. There is next a blue or blue-violet line of wave-length 470·2; and last, in the less easily visible part of the spectrum, there are five strong violet lines, of which the fourth, which is the most brilliant, has the wave-length 420·0.

Unfortunately, the red lines, which are not to be mistaken for those of any other substance, are not easily seen when a jar discharge is passed through argon at atmospheric pressure, unless a large jar and a very powerful current be employed. The spectrum seen under these conditions has been examined by Professor Schuster. The most characteristic lines are perhaps those in the neighbourhood of F, and are very easily seen if there be not too much nitrogen, in spite of the presence of some oxygen and water vapour. The approximate wave-lengths are—

487·91	Strong.
[486·07]	F.
484·71	Not quite so strong.
480·52	Strong.
476·50	Fairly strong characteristic triplet.
473·53	
472·56	

It is necessary to anticipitate Mr. Crookes' communication, and to state that when the current is passed from the induction coil in one direction, that end of the capillary tube next the positive pole appears of a redder, and that next the negative pole of a bluer hue. There are, in effect, two spectra, which Mr. Crookes has succeeded in separating to a considerable extent. Mr. E. C. C. Baly,[*] who has noticed a similar phenomenon, attributes it to the presence of two gases. He says:—"When an electric current is passed through a mixture of two gases, one is separated from the other and appears in the negative glow." The conclusion would follow that what we have termed "argon" is in reality a mixture of two gases which have as yet not been separated. This conclusion, if true, is of great importance, and experiments are now in progress to test it by the use of other physical methods. The full bearing of this possibility will appear later.

The presence of a small quantity of nitrogen interferes greatly with the argon spectrum. But we have found that in a tube with platinum electrodes, after the discharge has been passed for four hours, the spectrum of nitrogen disappears, and the argon spectrum manifests itself in full purity. A specially constructed tube with magnesium electrodes, which we hoped would yield good results, removed all traces of nitrogen, it is true; but hydrogen was evolved from the magnesium, and showed its characteristic lines very strongly. However, these are easily identified. The gas evolved on heating magnesium *in vacuo*, as proved by a separate experiment, consists entirely of hydrogen.

Mr. Crookes has established the identity of the chief lines of the spectrum of gas separated from air-nitrogen by aid of magnesium with that remaining after sparking the air-nitrogen with oxygen in presence of caustic soda solution.

Professor Schuster also has found the principal lines identical in the spectra of the two gases, as observed by the jar discharge at atmospheric pressure.

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[*] 'Proc. Phys. Soc.,' 1893, p. 147.

XII. *Solubility of Argon in Water.*

Determinations of the solubility in water of argon, prepared by sparking, gave 3·94 volumes per 100 of water at 12°. The solubility of gas prepared by means of magnesium was found to be 4·05 volumes per 100 at 13·9°. The gas is therefore about 2½ times as soluble as nitrogen, and possesses approximately the same solubility as oxygen.

The fact that argon is more soluble than nitrogen would lead us to expect it in increased proportion in the dissolved gases of rain water. Experiment has confirmed this anticipation. "Nitrogen" prepared from the dissolved gases of water supplied from a rain-water cistern was weighed upon two occasions. The weights, corresponding to those recorded in § I, were 2·3221 and 2·3227, showing an excess of 24 milligrams above the weight of true nitrogen. Since the corresponding excess for "atmospheric nitrogen" is 11 milligrams, we conclude that the water "nitrogen" is relatively more than twice as rich in argon.

On the other hand, gas evolved from the hot spring at Bath, and collected for us by Dr. A. Richardson, gave a residue after removal of oxygen and carbonic acid, whose weight was only about midway between that of true and atmospheric nitrogen.

XIII. *Behaviour at Low Temperatures.*[*]

Preliminary experiments, carried out to liquefy argon at a pressure of about 100 atmospheres, and at a temperature of −90°, failed. No appearance of liquefaction could be observed.

Professor Charles Olszewski, of Cracow, the well-known authority on the constants of liquefied gases at low temperatures, kindly offered to make experiments on the liquefaction of argon. His results are embodied in a separate communication, but it is allowable to state here that the gas has a lower critical temperature (−121°) and a lower boiling point (−187°) than oxygen, and that he has succeeded in solidifying argon to white crystals, melting at −189·6°. The density of the liquid is approximately 1·5, that of oxygen being 1·124, and of nitrogen 0·885. The sample of gas he experimented with was exceptionally pure, and had been prepared by help of magnesium. It showed no trace of nitrogen when examined in a vacuum tube.

XIV. *Ratio of Specific Heats.*

In order to decide regarding the elementary or compound nature of argon, experiments were made on the velocity of sound in it. It will be remembered that, from the velocity of sound in a gas, the ratio of specific heat at constant pressure to that at constant volume can be deduced by means of the equation

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[*] The arrangements for the experiments upon this branch of the subject were left entirely in Professor Ramsay's hands.

$$n\lambda = v = \sqrt{\left\{\frac{e}{d}(1+\alpha t)\frac{C_p}{C_v}\right\}},$$

when n is the frequency, λ the wave-length of sound, v its velocity, e the isothermal elasticity, d the density, $(1+\alpha t)$ the temperature correction, C_p the specific heat at constant pressure, and C_v that at constant volume. In comparing two gases at the same temperature, each of which obeys Boyle's law with sufficient approximation, and in using the same sound, many of these factors disappear, and the ratio of specific heats of one gas may be deduced from that of the other, if known, by means of the proportion

$$\lambda^2 d : \lambda'^2 d' :: 1{\cdot}41 : x,$$

where, for example, λ and d refer to air, of which the ratio is 1·41, according to observations by Röntgen, Wüllner, Kayser, and Jamin and Richard.

Two completely different series of observations, one in a tube of about 2 mm. diameter, and one in one of 8 mm., made with entirely different samples of gas, gave, the first, 1·65 as the ratio, and, the second, 1·61.

Experiments made with the first tube, to test the accuracy of its working, gave for carbon dioxide the ratio 1·276, instead of 1·288, the mean of all previous determinations; and the half wave-length of sound in hydrogen was found to be 73·6, instead of 74·5, the mean of those previously found. The ratio of the specific heats of hydrogen found was 1·39, instead of 1·402.

There can be no doubt, therefore, that argon gives practically the ratio of specific heats, viz., 1·66, proper to a gas in which all the energy is translational. The only other gas which has been found to behave similarly is mercury gas, at a high temperature.*

XV. *Attempts to induce Chemical Combination.*

Many attempts to induce argon to combine will be described in full in the complete paper. Suffice it to say here, that all such attempts have as yet proved abortive. Argon does not combine with oxygen in presence of alkali under the influence of the electric discharge, nor with hydrogen in presence of acid or alkali also when sparked; nor with chlorine, dry or moist, when sparked; nor with phosphorus at a bright-red heat, nor with sulphur at bright redness. Tellurium may be distilled in a current of the gas; so may sodium and potassium, their metallic lustre remaining unchanged. It is unabsorbed by passing it over fused red-hot caustic soda, or soda-lime heated to bright redness; it passes unaffected over fused and bright red-hot potassium nitrate; and red-hot sodium peroxide does not combine with it. Persulphides of sodium and calcium are also without action at a red heat. Platinum black does not absorb it, nor does platinum sponge, and wet oxidising and chlorinating agents, such as nitro-hydrochloric acid, bromine water, bromine and alkali, and hydrochloric acid and potassium permanganate, are entirely without action. Experiments with fluorine are in contemplation, but the difficulty is great; and an attempt will be made to produce a carbon arc in the gas. Mixtures of sodium and silica and of sodium and boracic anhydride are also without action, hence it appears to resist attack by nascent silicon and by nascent boron.

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* Kundt and Warburg, 'Pogg. Ann.,' vol. 157, p. 353 (1876).

XVI. *General Conclusions.*

It remains, finally, to discuss the probable nature of the gas, or mixture of gases, which we have succeeded in separating from atmospheric air, and which has been provisionally named *argon.*

The presence of argon in the atmosphere is proved by many lines of evidence. The higher density of "atmospheric nitrogen," and the uniformity in the density of samples of chemical nitrogen prepared from different compounds, lead to the conclusion that the cause of the anomaly is the presence of a heavy gas in air. If that gas possess the density 20 compared with hydrogen, "atmospheric nitrogen" should contain of it approximately 1 per cent. This is, in fact, found to be the case. Moreover, as nitrogen is removed from air by means of red-hot magnesium, the density of the remaining gas rises proportionately to the concentration of the heavier constituent.

Second. This gas has been concentrated in the atmosphere by diffusion. It is true that it cannot be freed from oxygen and nitrogen by diffusion, but the process of diffusion increases, relatively to nitrogen, the amount of argon in that portion which does not pass through the porous walls. This has been proved by its increase in density.

Third. As the solubility of argon in water is relatively high, it is to be expected that the density of the mixture of argon and nitrogen, pumped out of water along with oxygen, should, after the removal of the oxygen, exceed that of "atmospheric nitrogen." Experiment has shown that the density is considerably increased.

Fourth. It is in the highest degree improbable that two processes, so different from each other, should manufacture the same product. The explanation is simple if it be granted that these processes merely eliminate nitrogen from an atmospheric mixture. Moreover, if, as appears probable, argon be an element, or a mixture of elements, its manufacture would mean its separation from one of the substances employed. The gas which can be removed from red-hot magnesium in a vacuum has been found to be wholly hydrogen. Nitrogen from chemical sources has been practically all absorbed by magnesium, and also when sparked in presence of oxygen; hence argon cannot have resulted from the decomposition of nitrogen. That it is not produced from oxygen is sufficiently borne out by its preparation by means of magnesium.

Other arguments could be adduced, but the above are sufficient to justify the conclusion that argon is present in the atmosphere.

The identity of the leading lines in the spectrum, the similar solubility and the similar density, appear to prove the identity of the argon prepared by both processes.

That argon is an element, or a mixture of elements, may be inferred from the observations of § XIV. For Clausius has shown that if K be the energy of translatory motion of the molecules of a gas, and H their whole kinetic energy, then

$$\frac{K}{H} = \frac{3(C_p - C_v)}{2C_v},$$

C_p and C_v denoting as usual the specific heat at constant pressure and at constant volume respectively. Hence if, as for mercury vapour and for argon (§ XIV), the ratio of specific heats $C_p : C_v$ be $1\frac{2}{3}$, it follows that $K = H$, or that the whole kinetic energy of the gas is accounted for by the translatory motion of its molecules. In the case of mercury the absence of interatomic energy is regarded as proof of the monatomic character of the vapour, and the conclusion holds equally good for argon.

The only alternative is to suppose that if argon molecules are di- or polyatomic, the atoms acquire no relative motion, even of rotation, a conclusion improbable in itself and one postulating the sphericity of such complex groups of atoms.

Now a monatomic gas can be only an element, or a mixture of elements; and hence it follows that argon is not of a compound nature.

From Avogadro's law, the density of a gas is half its molecular weight; and as the density of argon is approximately 20, hence its molecular weight must be 40. But its molecule is identical with its atom; hence its atomic weight, or, if it be a mixture, the mean of the atomic weights of that mixture, taken for the proportion in which they are present, must be 40.

There is evidence both for and against the hypothesis that argon is a mixture: for, owing to Mr. Crookes' observations of the dual character of its spectrum; against, because of Professor Olszewski's statement that it has a definite melting point, a definite boiling point, and a definite critical temperature and pressure; and because on compressing the gas in presence of its liquid, pressure remains sensibly constant until all gas has condensed to liquid. The latter experiments are the well-known criteria of a pure substance; the former is not known with certainty to be characteristic of a mixture. The conclusions which follow are, however, so startling, that in our future experimental work we shall endeavour to decide the question by other means.

For the present, however, the balance of evidence seems to point to simplicity. We have therefore to discuss the relations to other elements of an element of atomic weight 40. We inclined for long to the view that argon was possibly one or more than one of the elements which might be expected to follow fluorine in the periodic classification of the elements—elements which should have an atomic weight between 19, that of fluorine, and 23, that of sodium. But this view is apparently put out of court by the discovery of the monatomic nature of its molecules.

The series of elements possessing atomic weights near 40 are:—

Chlorine	35·5
Potassium	39·1
Calcium	40·0
Scandium..........	44·0

There can be no doubt that potassium, calcium, and scandium follow legitimately their predecessors in the vertical columns, lithium, beryllium, and boron, and that they are in almost certain relation with rubidium, strontium, and (but not so certainly) yttrium. If argon be a single element, then there is reason to doubt whether the periodic classification of the elements is complete; whether, in fact, elements may not exist which cannot be fitted among those of which it is composed. On the other hand, if argon be a mixture of two elements, they might find place in the eighth group, one after chlorine and one after bromine. Assuming 37 (the approximate mean between the atomic weights of chlorine and potassium) to be the atomic weight of the lighter element, and 40 the mean atomic weight found, and supposing that the second element has an atomic weight between those of bromine, 80, and rubidium, 85·5, viz., 82, the mixture should consist of 93·3 per cent. of the lighter, and 6·7 per cent. of the heavier element. But it appears improbable that such a high percentage as 6·7 of a heavier element should have escaped detection during liquefaction.

If it be supposed that argon belongs to the eighth group, then its properties would fit fairly well with what might be anticipated. For the series, which contains

$$\mathrm{Si}_x{}^{\mathrm{IV}},\quad \mathrm{P}_4{}^{\mathrm{III\ and\ V}},\quad \mathrm{S}_{8\text{ to }2}^{\mathrm{II\ to\ VI}},\quad \text{and } \mathrm{Cl}_2{}^{\mathrm{I\ to\ VII}},$$

might be expected to end with an element of monatomic molecules, of no valency, *i.e.*, incapable of forming a compound, or if forming one, being an octad; and it would form a possible transition to potassium, with its monovalence, on the other hand. Such conceptions are, however, of a speculative nature; yet they may be perhaps excused, if they in any way lead to experiments which tend to throw more light on the anomalies of this curious element.

In conclusion, it need excite no astonishment that argon is so indifferent to reagents. For mercury, although a monatomic element, forms compounds which are by no means stable at a high temperature in the gaseous state; and attempts to produce compounds of argon may be likened to attempts to cause combination between mercury gas at 800° and other elements. As for the physical condition of argon, that of a gas, we possess no knowledge why carbon, with its low atomic weight, should be a solid, while nitrogen is a gas, except in so far as we ascribe molecular complexity to the former and comparative molecular simplicity to the latter. Argon, with its comparatively low density and its molecular simplicity, might well be expected to rank among the gases. And its inertness, which has suggested its name, sufficiently explains why it has not previously been discovered as a constituent of compound bodies.

We would suggest for this element, assuming provisionally that it is not a mixture, the symbol A.

We have to record our thanks to Messrs. Gordon, Kellas, and Matthews, who have materially assisted us in the prosecution of this research.

Addendum by Professor RAMSAY, March 20, 1895.

Further determinations have been made of the density of argon prepared by means of magnesium. The mean result of six very concordant weighings of different samples, in which every care was taken in each case to circulate the argon over magnesium for hours after all contraction had ceased, gave the density 19·90.

The value of R in the gas-equation $R = pv/T$ has been carefully determined for argon, at temperatures determined by means of a thermometer filled with pure hydrogen. I have found that the value of R remains practically constant between −87° and +248°; the greatest difference between the extreme values of R amounts to only 0·3 per cent. Argon, therefore, behaves as a "perfect" gas, and shows no sign of association on cooling, nor of dissociation on heating.

The ratio of the specific heat at constant volume to that at constant pressure has been reinvestigated; the mean of four very concordant determinations with distinct samples of argon is 1·645.

The molecular weight of argon is therefore 39·8, and the same number expresses its atomic weight, unless it be a mixture of two elements, or of mono- and diatomic molecules of the same element. The ratio of specific heats might support the last supposition; but the thermal behaviour of the gas lends no support to this view.

II. "On the Spectra of Argon." By WILLIAM CROOKES, F.R.S., &c. Received January 26, 1895.

(Abstract.)

Through the kindness of Lord Rayleigh and Professor Ramsay I have been enabled to examine the spectrum of this gas in a very accurate spectroscope, and also to take photographs of its spectra in a spectrograph fitted with a complete quartz train.

Argon resembles nitrogen in that it gives two distinct spectra according to the strength of the induction current employed. But while the two spectra of nitrogen are different in character, one showing fluted bands and the other sharp lines, the argon spectra both consist of sharp lines. It is, however, very difficult to get argon so free from nitrogen that it will not at first show the nitrogen flutings superposed on its own special system of lines. I have used argon prepared by Lord Rayleigh, Professor Ramsay, and myself, and, however free it was supposed to be from nitrogen, I could always detect the nitrogen bands in its spectrum. These, however, disappear when the induction spark is passed through the tube for some time, varying from a few minutes to a few hours. The vacuum tubes best adapted for showing the spectra are of the ordinary Plücker form, having a capillary tube in the middle. For photographing the higher rays which are cut off by glass I have used a similar tube, "end on," having a quartz window at one end.

The pressure of argon giving the greatest luminosity and most brilliant spectrum is 3 mm.

If the pressure is further reduced, and a Leyden jar intercalated in the circuit, the colour of the luminous discharge changes from red to a rich steel blue, and the spectrum shows an almost entirely different set of lines.

I have taken photographs of the two spectra of argon partly superposed. In this way their dissimilarity is readily seen.[*] In the spectrum of the blue glow I have counted 119 lines, and in that of the red glow 80 lines, making 199 in all. Of these 26 appear to be common to both spectra.

I have said that the residual nitrogen is removed by sparking the tube for some time when platinum terminals are sealed in. This is not the only way of purifying the argon. By the kindness of Professor Ramsay, I was allowed to take some vacuum tubes to his laboratory and there exhaust and fill them with some of his purest argon. On this occasion I simultaneously filled, exhausted, and sealed off two Plücker tubes, one having platinum and the other aluminium terminals. On testing the gas immediately after they were sealed off, each tube showed the argon spectrum, contaminated by a trace of nitrogen bands. The next day the tube with platinum terminals was unchanged, but that having aluminium terminals showed the pure spectrum of argon, the faint nitrogen bands having entirely disappeared during the night. After an hour's sparking the tube with platinum terminals likewise gave a pure argon spectrum.

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[*] Photographs of the different spectra of argon, and other gaseous spectra for comparison, were projected on the screen.

A vacuum tube was filled with pure argon and kept on the pump while observations were made on the spectrum of the gas as exhaustion proceeded. The large coil was used with a current of 8·84 ampères and 11 volts, no jar was interposed. At a pressure of 3 mm. the spectrum was that of the pure red glow of argon. This persisted as the exhaustion rose, until, at a pressure of about half a millimeter, flashes of blue light made their appearance. At a quarter of a millimeter the colour of the ignited gas was pure blue, and the spectrum showed no trace of the red glow.

An experiment was now made to see if the small quantity of argon normally present in the atmosphere could be detected without previous concentration. Nitrogen was prepared from the atmosphere by burning phosphorus, and was purified in the usual manner. This gas, well dried over phosphoric anhydride, was passed into a vacuum tube, the air washed out by two fillings and exhaustions, and the tube was finally sealed off at a pressure of 52 mm. It was used for photographing the band spectrum of nitrogen on several occasions, and altogether it was exposed to the induction current from the large coil for eight hours before any change was noticed. The last time when photographing its spectrum difficulty was experienced in getting the spark to pass, so I increased the current and intercalated a small jar. The colour immediately changed from the reddish-yellow of nitrogen to the blue of argon, and on applying the spectroscope the lines of argon shone out with scarcely any admixture of nitrogen bands. With great difficulty, and by employing a very small jar, I was able to take one photograph of its spectrum and compare it with the spectrum of argon from Professor Ramsay, both being taken on the same plate. The argon spectra were the same in each. After this it became difficult to illuminate the tube, except by employing a dangerously large current. Whenever a flash passed it was of a deep blue colour. Assuming that the atmosphere contains 1 per cent. of argon, the 3 mm. of nitrogen originally in the tube would contain 0·03 mm.

of argon. After the nitrogen had been absorbed by the spattered platinum this pressure of argon would be near the point of non-conduction.

In all cases, when argon has been obtained in this manner, the spectrum has been that of the blue-glowing gas. It is not improbable, and I understand that independent observations have already led both the discoverers to the same conclusion, that the gas argon is not a simple body, but is a mixture of at least two elements, one of which glows red and the other blue, each having its distinctive spectrum. The theory that it is a simple body has, however, support from the analogy of other gases. Thus, nitrogen has two distinct spectra, one or the other being produced by varying the pressure and intensity of the spark. I have made vacuum tubes containing rarefied nitrogen, which show either the fluted band or the sharp line spectrum by simply turning the screw of the make-and-break, exactly as the two spectra of argon can be changed from one to the other.

I have prepared tubes containing other gases as well as nitrogen at different pressures, and have examined their spectra both by eye observations and by photography. The sharp line spectrum of nitrogen is not nearly so striking in brilliancy, number or sharpness of lines as are those of argon, and careful scrutiny fails to show more than one or two apparent coincidences between lines in the two spectra. Between the spectra of argon and the band spectrum of nitrogen there are two or three close approximations of lines, but a projection on the screen of a magnified image of the two spectra partly superposed will show that two at least of these are not real coincidences.

I have looked for indications of lines in the argon spectra corresponding to the corona line at 531·7, the aurora line at 557·1, and the helium line at 587·5, but have failed to detect any line of argon sufficiently near these positions to fall within the limits of experimental error.

I have found no other spectrum-giving gas or vapour yield spectra at all like those of argon, and the apparent coincidences in some of the lines, which on one or two occasions are noticed, have been very few, and would probably disappear on using a higher dispersion. As far, therefore, as spectrum work can decide, the verdict must be that Lord Rayleigh and Professor Ramsay have added one, if not two, members to the family of elementary bodies.

III. "The Liquefaction and Solidification of Argon." By Dr. K. OLSZEWSKI, Professor of Chemistry in the University of Cracow. Communicated by Professor WILLIAM RAMSAY, F.R.S. Received January 28, 1895.

(Abstract.)

Having been furnished, by Professor Ramsay's kindness, with a sample of the new gas, argon, I have carried out experiments on its behaviour at low temperatures and at high pressures, in order to contribute, at least in part, to the knowledge of the properties of this interesting body.

The argon with which I was supplied had been dried with phosphoric anhydride; its density was 19·9 ($H = 1$); and Professor Ramsay thought that at the outside it might contain 1 to 2 per cent. of nitrogen, although it showed no nitrogen spectrum when examined in a Plücker's tube.

For the first two experiments I made use of a Cailletet's apparatus. As cooling agent I used liquid ethylene, boiling under diminished pressure.

In both the other experiments the argon was contained in a burette, closed at both ends with glass stop-cocks. By connecting the lower end of the burette with a mercury reservoir, the argon was transferred into a narrow glass tube fused at its lower end to the upper end of the burette, and in which the argon was liquefied, and its volume in the liquid state measured. In these two series of experiments liquid oxygen, boiling under atmospheric or under diminished pressure, was employed as a cooling agent. I made use of a hydrogen thermometer in all these experiments to measure low temperatures.

Determination of the Critical Constants of Argon.

As soon as the temperature of the liquid ethylene had been lowered to —128°·6, the argon easily condensed to a colourless liquid under a pressure of 38 atmospheres. On slowly raising the temperature of the ethylene, the meniscus of the liquid argon became less and less distinct, and finally vanished.

From seven determinations the critical pressure was found to be 50·6 atmospheres; the mean of the seven estimations of the critical temperature is —121°.

At lower temperatures the following vapour-pressures were recorded:—

Expt.	Temperature.	Pressure.	Expt.	Temperature.	Pressure.
8	−128°·6	38·0 atmos.	13	−134°·4	29·8 atmos.
9	−129·6	35·8 „	14	−135·1	29·0 „
10	−129·4	35·8 „	15	−136·2	27·3 „
11	−129·3	35·8 „	16	−138·3	25 3 „
12	−129·6	35·8 „	17	−139·1	23·7 „

Determination of the Boiling and Freezing Points.

A calibrated tube, intended to receive the argon to be liquefied, and the hydrogen thermometer were immersed in boiling oxygen. On admitting argon, and diminishing the temperature of the liquid oxygen below −187°, the liquefaction of the argon became manifest. When liquefaction had taken place, I carefully equalised the pressure of the argon with that of the atmosphere, and regulated the temperature, so that the state of balance was maintained for a long time. This process gives the boiling point of argon under atmospheric pressure. Four experiments gave the numbers −186°·7, −186°·8, −187°·0, and −187°·3. The mean is −186°·9, which I consider to be the boiling point under atmospheric pressure (740·5 mm.).

The quantity of argon used for these experiments, reduced to normal temperature and pressure, was 99·5 c.c.; the quantity of liquid corresponding to that volume of gas was approximately 0·114 c.c Hence the density of argon at its boiling point may be taken as approximately. 1·5. This proves that the density of liquid argon at its boiling point (−187°) is much higher than that of oxygen, which I have found, under similar conditions, to be 1·124.

By lowering the temperature of the oxygen to −191° by slow exhaustion, the argon froze to a crystalline mass, resembling ice; on further lowering temperature it became white and opaque. When the temperature was raised it melted; four observations which I made to determine its melting point gave the numbers: −189°·0, −190°·6, −189°·6, and −189°·4. The mean of these numbers is −189°·6; and this may be accepted as the melting point of argon.

In the following table I have given a comparison of physical constants, in which those of argon are compared with those of other so-called permanent gases. The data are from my previous work on the subject.

As can be seen from the foregoing table, argon belongs to the so-called "permanent" gases, and, as regards difficulty in liquefying it, it occupies the fourth place, viz., between carbon monoxide and oxygen. Its behaviour on liquefaction places it nearest to oxygen, but it differs entirely from oxygen in being solidifiable; as is well known, oxygen has not yet been made to assume a solid state.

The high density of argon rendered it probable that its liquefaction would take place at a higher temperature than that at which oxygen liquefies. Its unexpectedly low critical temperature and boiling point seem to have some relation to its simple molecular constitution.

Name.	Critical temperature.	Critical pressure.	Boiling point.	Freezing point.	Freezing pressure.	Density of gas.	Density of liquid at boiling point.	Colour of liquid.
Hydrogen (H_2)	Below −220°·0	Atmos. 20·0	?	?	mm. ?	1·0	?	Colourless.
Nitrogen (N_2)	−146·0	35·0	−194°·4	−214°·0	60	14·0	0 885	„
Carbonic oxide (CO).....	−139·5	35·5	−190·0	−207·0	100	14 0	?	„
Argon (A_1).............	−121·0	50·6	−187·0	−189·6	?	19·9	About 1·5	„
Oxygen (O_2)...........	−118·8	50·8	−182·7	?	?	16·0	1·124	Bluish.
Nitric oxide (NO).......	− 93·5	71·2	−153·6	−167·0	138	15·0	?	Colourless.
Methane (CH_4).........	− 81·8	54 9	−164 0	−185·8	80	8·0	0·415	„

IV. "On the Spark Spectrum of Argon as it appears in the Spark Spectrum of Air." By W. N. Hartley, F.R.S., Professor of Chemistry, Royal College of Science, Dublin. Received January 31, 1895. [Revised February 18, 1895.]

The spark spectrum of air as photographed, mapped, and described by Hartley and Adeney* contains various lines which they have not been able to attribute to oxygen or to nitrogen, having no grounds for assigning them to one element or the other. The lines belonging to oxygen and nitrogen, when produced by uncondensed sparks, are comparatively well known; so also are the lines of oxygen when a condensed spark is used, but it is otherwise with the lines of nitrogen when the spark is condensed.

It was observed by one of us† that the very abundant air lines in the photographed spectrum of air were subject to considerable variations according to circumstances. Thus, by modifications in manipulation, the air-spectrum might almost be suppressed, though the metallic lines remained strong.

On the other hand, it was remarked that certain metallic electrodes gave spectra of air with some lines more prominent than others, also more sharply and distinctly defined. Aluminium and copper were two which gave the most distinctly-marked air-lines. Platinum also in some cases.

From statements contained in the paper contributed by Mr. Crookes on the spectrum of argon it may be gathered that the following conditions have been observed to yield the brightest and purest spectrum of argon:—

1. As to pressure, about 3 mm.
2. Electrodes of aluminium or platinum.
3. A condensed spark from a coil and Leyden jar.

These conditions were very nearly approached when Hartley and Adeney's spectra of air were photographed, thus:—

1. As argon is about 1 per cent. of the atmosphere, its partial pressure is about 7·5 mm.
2. Electrodes of aluminium, copper, platinum, and palladium were used.
3. A condensed spark was always employed, this being produced by using a coil and Leyden jar.

It was therefore reasonable to expect that the spectrum of the blue and ultra-violet rays emitted by argon would be recognised among some of the weaker lines in the spectrum of air. When operating in a tube closed by a plate of quartz with oxygen, carbon dioxide, nitrogen, and other gases, with carbon electrodes and points of aluminium, the variations in the spectrum were considered to be of the same nature as those described in the 'Roy. Soc. Proc.,' vol. 55, p. 344, 1894, but it appears now to be extremely probable that they were in some cases due to the removal of nitrogen and the development of the argon spectrum. Accordingly the spark spectrum of air has been examined[*] for those lines more refrangible than 4674, since, if it be an elementary form of matter, they must appear there, or some explanation should be given to account for their absence.

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* 'Phil. Trans.,' vol. 175, p. 63, 1884.

† 'Journ. Chem. Soc.,' vol. 41, p. 84, 1882; also 'Sci. Trans. Royal Dublin Soc.,' vol. 1, p. 231 (new series), 1881.

It may here be stated that the photographs were taken in 1882 with a Rutherford diffraction grating with 14,000 lines to the inch and quartz lenses of 36 inches focus. Two sets of measurements were made, one from the photograph of each metal; where the lines did not agree, sometimes a third set of measurements were made from another plate, but when this was not necessary the mean value was adopted as the wave-length. The maximum error for well-defined lines was believed to be not greater than ±0·3 of a tenth metre; for faint lines it is larger, but it seldom rises to more than 0·5. The metre of Ångström was our standard.

The following table gives the lines in the red and in the blue spectra of argon, and the corresponding lines in the spectrum of air. With the exception of two lines printed in italics, the latter are all absolute wave-lengths.

Column I.—The spectrum of the red rays of argon.
„ II.—The spectrum of the blue rays of argon.
„ III.—Hartley and Adeney's spectrum of air, with the non-coincident lines omitted.

I.	II.	III.	
4629·5		4628·9	Strong.
	4608	4605·6	Weak.
4594·5		4595·0	Weak.
	4586·9	4589·3	Weak.
	4543·5	4543·4	Faint.
4509·5	4509·5	4506·6	Weak.
4478·3		4476·6	Weak, fine.
4426·5		4425·9	Weak, nebulous.
4399·5		4402·6	Faint.
4376·5		4378·0	Faint.
4348·5		4348·2	Strong.
	4345·0	4343·9	Weak, fine.
4333·5	4333·5	4335·9	Faint.
		4330·8	Faint.

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[*] 'Phil. Trans.,' vol. 175, p. 63, 1884.

I.	II.	III.	
	4300·5	4302·0	Very faint.
4299·0			
4277·0		4275·3	Faint, nebulous.
4272·0	4272·0	4274·3	Very faint, sharp.
4266·0	4266·0	4265·4	Very faint.
4251·5	4251·5	4253·4	Faint.
4228·5		4228·9	Fairly strong, nebulous.
4201·0	4201·0	4206·3	Faint, nebulous.
4198·0	4198·0	4197·9	Faint, nebulous.
4191·5	4191·5	4189·3	Weak, sharp.
4183·0	4183·0	4185·1	Weak, sharp.
4164·5	4164·5	4169·2	Weak, nebulous.
4159·5	4159·5	4157·9	Very faint, nebulous.
	4131·5	4132·8	Fairly strong, fine.
	4105·0	4104·3	Fairly strong, fine.
		4102·6	Fairly strong, fine.
	4072·5	4071·4	Strong, fine.
	4033·0	4034·4	Weak, nebulous.
	3967·8	3967·3	Faint, fine.
	3943·5	3944·5	Faint, nebulous.
	3931·8	3932·9	Very faint.
	3928·5	3929·0	Very faint.
	3892·0	3892·4	Very faint.
	3851·5	3850·0	Faint, fine.
	3803·5	3804·0	Faint, fine.
(3771·5)	3780·8	3782·1	Faint, fine.
	3770·5	3771·5	Faint, fine.
	3738·5	3739·7	Very faint, fine.
	3729·8	3726·6	Strong, fine.
	3587·0	3589·6	Weak, fine.
	3580·3	3583·7	Weak, fine.
	3575·0	3576·2	Weak, fine.
	3560·0	3560·6	Weak, nebulous.
	3544·5	3544·2	Weak, nebulous.
	3513·5	*3514·1*	Very faint, fine.
	3490·0	3490·7	Weak, fine.
	3475·7	3478·1	Faint, fine.
	3453·5	*3456·2*	Very faint, fine.
	3388·0	3389·9	Fairly strong, fine.
	3042·7	3042·5	Faint, fine.
	2734·5	2733·2	Faint, fine. Strongest in centre, thinning away at each end.

It will appear that these lines, which approximate so closely to a large number of lines in the argon spectrum, are scarcely likely to do so by mere chance. There is one group marked by a bracket which may be identified in the spectrum of aluminium in the 'J. Chem. Soc.,' vol. 41, p. 90 (photographs). It will be seen that, compared with others, the spectrum of aluminium No. 4 is rich in well-defined air lines, and the group of lines referred to is that lying immediately below the space between the most prominent lines in the first or least-refrangible triplet in the spectrum of cadmium No. 3. They are well seen also in the indium spectrum No. 5 from 3850 to 3514·1. The enlargements of these prismatic spectra, however, cannot compare with the grating spectra, the air lines being, as far as possible, suppressed in the former.

I do not attribute much importance to the fact that argon gives two spectra; the red appears to be the spectrum of the first order, or the spectrum of the lower temperature which corresponds thereto; the blue is the line spectrum, or spectrum at the higher temperature.

I have photographed simultaneously from the same spark the two spectra of nitrogen as rendered by atmospheric air.

It is therefore more likely that argon is one substance and not two. Whether it is a compound or an element is a question into which the following considerations may enter. There are at present no gaseous substances known which can withstand the temperature of the condensed spark without exhibiting the spectra of one or other of the elements of which it is composed. If, therefore, argon were N_3 it would disclose the spectrum of nitrogen. As the spectrum is not that of any known substance, it follows that, if a compound, it must be a compound of a new element.

A Letter from Prof. FITZGERALD upon the Atomicity of Argon was read.

Presents, January 31, 1895.

Transactions.

Brussels:—Association Belge des Chimistes. Bulletin. Année VII. No. 8. 8vo. *Bruxelles* 1893. The Association.

Bucharest:—Societăţii de Sciinţe Fizice. Buletinul. Anul III. Nos. 5—8. Centenarul lui Lavoisier, 1794–1894. 4to. *Bucuresci* 1895. The Society.

Cracow:—Académie des Sciences. Bulletin International. Décembre. 8vo. *Cracovie* 1894. The Academy.

Edinburgh:—Royal Scottish Society of Arts. Transactions. Vol. XIII. Part 4. 8vo. *Edinburgh* 1894. The Society.

Royal Society. List of Members. 1894. 4to. [*Edinburgh.*] The Society.

Göttingen:—Königl. Gesell. der Wissenschaften. Nachrichten. 1894. No. 4. 8vo. *Göttingen* 1895. The Society.

Heidelberg:—Naturhistorisch-Medicinscher Verein. Verhandlungen. Neue Folge. Band 5. Heft 3. 8vo. *Heidelberg* 1894. The Society.

Transactions (*continued*).

London:—British Museum. Catalogue of Arabic Books. Vol. I. 4to. *London* 1894; Catalogue of Hebrew Books acquired during the years 1868–92. 4to. *London* 1894.
The Trustees.

Pathological Society. Transactions. Vol. XLV. 8vo. *London* 1894. The Society.

Pharmaceutical Society. Calendar, 1895. 8vo. *London* 1895.
The Society.

Royal United Service Institution. Journal. January, 1895. 8vo. *London.* The Institution.

Sanitary Institute. Journal. Vol. XV. Part 4. 8vo. *London* 1895. The Institute.

Louvain:—L'Université Catholique. Annuaire 1895. 12mo. *Louvain* [1894]; Programme des Cours. Année Académique. 1894–95. 8vo. *Louvain* 1894; Theses. 8vo. *Louvain* 1894; and Sundry Excerpts from the 'Revue Catholique.' 8vo.
The University.

Naples:—Accademia Pontaniana. Atti. Vol. XXIV. 4to. *Naples* 1894. The Academy.

Accademia delle Scienze Fisiche e Matematiche. Rendiconto. Anno XXXIII. Fasc. 11, 12. 4to. *Napoli* 1894.
The Academy.

Santiago:—Sociedad Nacional de Mineria. Boletin. Año XI. No. 71. 4to. *Santiago de Chile* 1894. The Society.

Stavanger:—Museum. Aarsberetning. 1893. 8vo. *Stavanger* 1894. The Museum.

Turin:—Specola Vaticana. Pubblicazioni. Vol. IV. 4to. *Torino* 1894. The Observatory.

Vienna:—K.K. Naturhistorisches Hofmuseum. Annalen. Band IX. 4to. *Wien* 1894. The Museum.

Zoologisch-Botanische Gesellschaft. Verhandlungen. Band XLIV. III and IV Quartal. 8vo. *Wien* 1895.
The Society.

Zurich:—Naturforschende Gesellschaft. Vierteljahrsschrift. Jahrgang 39. Hefts 3 and 4. 8vo. *Zurich* 1894; Neujahrsblatt XCVII. Jahr 1895. 8vo. *Zurich* 1894. The Society.

Observations and Reports.

Brussels:—Académie Royale des Sciences. Annuaire 1895. 8vo. *Bruxelles.* The Academy.

Observations and Reports (*continued*).

Cambridge, Mass.:—Astronomical Observatory, Harvard College. Forty-ninth Annual Report. 8vo. *Cambridge* 1894. The Observatory.

Museum of Comparative Zoology. Annual Report of the Curator for 1893–94. 8vo. *Cambridge* (*Mass.*) 1894.

India:—Botanical Survey. Records. Vol. I. Nos. 3, 4. 8vo. *Calcutta* 1894. The Survey.

Trigonometrical Survey. Spirit-Levelled Heights. No. 1. Burma. Season 1892–93. 8vo. *Dehra Dun* 1894. The Survey.

London:—Board of Trade. Circular from the Board of Trade to the Railway Companies of the United Kingdom on the subject of Colour Vision. Folio. *London* 1894. H.M. Stationery Office.

Meteorological Office. Daily Weather Reports. July—December, 1893. 4to. *London;* Meteorological Observations at Stations of the Second Order. 1880. 4to. *London* 1894. The Office.

Madrid:—Observatorio. Observaciones Meteorológicas. 1892–93. 8vo. *Madrid* 1894. The Observatory.

Melbourne:—Observatory. Record of Results of Observations in Meteorology and Terrestrial Magnetism. April—June, 1894. 8vo. *Melbourne.* The Observatory.

Washington:—U. S. Department of Agriculture. Experiment Station Record. Vol. VI. No. 1. 8vo. *Washington* 1894. The Department.

Weather Bureau. Monthly Weather Review. September, 1894. 4to. *Washington.* The Bureau.

Journals.

Agricultural Gazette of New South Wales. Vol. V. Part 2. 8vo. *Sydney* 1894. Department of Agriculture, Sydney.

Astrophysical Journal. Vol. I. No. 1. 8vo. *Chicago* 1895. The Editors.

University Studies. Vol. II. No. 1. 8vo. *Lincoln, Nebraska.* University of Nebraska.

Delebecque (A.) Atlas des Lacs Français. [Planches 8—10]; [And 7 Excerpts. 8vo. and 4to.] The Author.

Hale (G. E.) On some attempts to Photograph the Solar Corona without an Eclipse. 8vo. [*Chicago*] [1894]. [With an Excerpt.] The Author.

Muybridge (Eadweard) Descriptive Zoopraxography, or the Science of Animal Locomotion. 8vo. *Chicago* 1893. The Author.

Spencer (J. W.) Reconstruction of the Antillean Continent. 8vo. *Rochester, U.S.A.* 1895. The Author.

Van Erden (F. W.) Flora Batava. Aflev. 307, 308. 4to. *Leiden* [1894]. The Netherlands Government.

Whitehead (John) North Derbyshire Mosses. 8vo. *Oldham* 1894. The Author.

February 7, 1895.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Oscillations of a Rotating Ellipsoidal Shell containing Fluid." By S. S. Hough. Communicated by Sir R. S. Ball, F.R.S. Received January 18, 1895.

(Abstract.)

The paper contains an application of the analysis used by M. Poincaré, in his memoir "Sur l'équilibre d'une masse fluide animée d'un mouvement de rotation,"* to the determination of the free oscillations of a system consisting of a fluid mass contained within a rigid ellipsoidal envelope, rotating about one of its principal axes. It is found that, when such a system is oscillating in one of its fundamental modes, the disturbances of the fluid are all expressible by means of Lamé functions, the functions involved being all of the same order; and a method of obtaining the frequencies of these oscillations, similar to that used by M. Poincaré for a fluid ellipsoid with a free surface, is given.

The oscillations, however, which involve Lamé functions of the second order, demand exceptional treatment in consequence of the fact that these alone imply any disturbance of the containing shell. Poincaré's analysis, with slight modifications to adapt it to the problem in hand, enables us to determine an expression for the fluid pressure at all points on the boundary in terms of the disturbances communicated to the shell. From this, the couples on the shell, due to the fluid pressure, are estimated and introduced into the dynamical equations of motion of the shell. A frequency equation of the 6th degree is derived, apparently involving three fundamental modes of oscillation. The equation, however, is found to be satisfied by the frequency of rotation of the system, and the corresponding oscillation is shown not to be real but to arise (analytically) only in consequence of the motion of the axes of reference. We are left with two fundamental modes.

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* 'Acta Mathematica,' vol. 7.

The case where the inertia of the shell is negligible, compared with that of the fluid, is of analytical interest, and can be approximately realised physically by means of a liquid gyrostat ('Nature,' vol. 15, p. 297) mounted in such a way that its centre of gravity is held at rest. When the axis of rotation is an axis of symmetry, the roots of the frequency equation will be real, and the motion therefore stable, either when this axis is the least axis, or when it exceeds three times the equatorial radius. When, however, the figure is not one of revolution, the analytical conditions of stability are not so simply expressible, but they will always be satisfied when the axis of rotation is the least axis, or when it exceeds three times either of the other axes.

On taking into account the inertia of the shell, the discussion is confined to the case where the ellipsoid is approximately spherical, and the solutions of the frequency equation then assume a simple form. Of the two modes of oscillations, the motion of the shell in one is analogous to the motion of a rigid body when slightly disturbed from a motion of rotation about a principal axis, but the period is found to be shorter than it would be were the fluid solidified; the other exists only in consequence of the contained fluid.

The former of these presents the greater interest. It has been supposed that if the axis of rotation of the earth were displaced from its axis of figure, an oscillatory motion would ensue which would give rise to a variation in the latitude of places on the earth's surface in a period of 305 days. Recent observations (*vide* Chandler, 'Astronomical Journal,' vols. 11, 12) have proved that such an oscillation is taking place, but that the theoretical estimate of the period is considerably too short. This paper was undertaken with the object of investigating whether the extension of the period could be explained by supposing that the earth possessed a fluid interior, in accordance with a suggestion made by M. Folie ('Acta Mathematica,' vol. 16). It is shown that the hypothesis of a fluid interior leads to a result directly opposite to that which observation requires, and that, therefore, the discovery of the variations of latitude so far from establishing the existence of a fluid interior, as supposed by M. Folie, rather affords an additional reason for discarding this hypothesis.

II. "On the Abelian System of Differential Equations, and their Rational and Integral Algebraic Integrals, with a Discussion of the Periodicity of Abelian Functions." By Rev. W. R. WESTROPP ROBERTS. Communicated by Rev. G. SALMON, D.D., F.R.S. Received January 17, 1895.

(Abstract.)

Before entering on the discussion of the Abelian system of differential equations, I treat of some general algebraic theorems having reference to the differences of various sets of "facients," and give a wider definition to the term "source," hitherto used to signify the source of a covariant, and treat of two operators, δ and Δ.

I then show how, by forming what I call a "square-matrix," all the conditions can be obtained which are fulfilled when a polynomial $f(2)$ of the degree $2n$ in z is a perfect square. With regard to these conditions, I remark that any one of them being given all the others can be found by successive operations of the operator δ.

I next treat of the system of differential equations termed "Abelian," in which there are m quantities and $m-1$ equations, comprehended in the typical form

$$\Sigma \frac{z^i dz}{\sqrt{f(z)}} = 0,$$

where Σ relates to the m quantities $z_1, z_2, \ldots, z_m$, and i may have any integer value from $i = 0$ to $i = m-2$, it being understood that $f(z)$ is a polynomial of the degree $2m$ in z; and I show that, if $f(z) \equiv z^{2m} + P_1 z^{2m-1} + P_2 z^{2m-2} + \ldots. P_{2m}$, be reduced to the degree $2m-2$ in z in the following manner—

$$f(z) + \{\phi(z)\}^2 - 2\phi(z) . L(z) \equiv F(z),$$

where

$$\phi(z) \equiv (z-z_1)(z-z_2) \ldots. (z-z_m) \equiv z^m + p_1 z^{m-1} + \ldots. p_m,$$

and $$L(z) = z^m + \frac{P_1}{2} z^{m-1} + \lambda_2 z^{m-2} + \lambda_3 z^{m-3} + \ldots. + \lambda_m,$$

$\lambda_2, \lambda_3, \ldots, \lambda_m$ being $m-1$ arbitrary constants, all the rational and integral algebraic integrals of the Abelian system

$$\Sigma \frac{z^i dz}{\sqrt{f(z)}} = 0$$

are immediately found by forming the "square-matrix" for F (z) and so obtaining the conditions that F (z) should be a perfect square. The various relations so found connecting the quantities $p_1, p_2, \ldots, p_m$, and $m-1$ arbitrary quantities $\lambda_2, \ldots, \lambda_m$, are algebraic integrals of the above system of differential equations, and are all *rational* and *integral.* I then apply the general theorem to the case $m = 2$, or the case of elliptic integrals, and easily deduce the result given by Cayley in his work entitled an 'Elementary Treatise on Elliptic Functions' (p. 340).

I next apply the theory to the case of $m = 3$, and deduce two algebraic integrals, and show how the remaining relations may be found, and lastly to the case $m = 4$.

The next subject treated of is the source of F (z), from which we derive a differential equation which I call the *fundamental equation* in the theory of Abelian integrals and functions, as its integral leads us to a form which, when operated on by δ, leads us to a new algebraic equation, which again leads to another by a second application of the operator. By this method I obtain a number of interesting results, many of which are now given for the first time, as far as I am aware.

I then define Abelian functions and, by a method of treatment depending on what precedes, show that they are periodic functions and determine their periods.

We have at first sight $2m-1$ independent periods, and I reduce them to $2m-2$ by an easy application of the foregoing theory.

The above is a short abstract of what my paper contains, the most important portions of it being (*a*) the determination of the algebraic integrals in a *rational* and *integral* form; (*b*) the easy proof of the periodicity of Abelian functions.

I omit from this paper a discussion of the case in which the number of variables exceeds m, as likely to make my communication too lengthy.

III. "On the Application of the Kinetic Theory to Dense Gases." By S. H. BURBURY, F.R.S. Received January 12, 1895.

(Abstract.)

1. Start with Clausius' equation

$$\tfrac{3}{2}pV = T_r + \tfrac{1}{2}\Sigma\Sigma Rr,$$

in which p denotes pressure per unit of area, V volume, and T_r kinetic energy of relative motion. Also R is the repulsive force, r the distance between the centres of two spheres, and the summation includes all pairs.

2. Evaluate $\Sigma\Sigma\mathrm{R}r$, on the assumption that no forces act except during collisions. That gives

$$\Sigma\Sigma\mathrm{R}r = \tfrac{2}{3}\pi c^3\rho \,.\, 2\rho\mathrm{T}_r,$$

c being the diameter of a sphere and ρ the number of spheres in unit of volume.

Let $$\tfrac{2}{3}\pi c^3\rho = \kappa.$$

Then $$\Sigma\Sigma\mathrm{R}r = \kappa \,.\, 2\rho\mathrm{T}_r,$$

and $$p = \tfrac{2}{3}(1+\kappa)\,\rho\mathrm{T}_r.$$

3. This suggests that we should take for our law of distribution of energy, not $\epsilon^{-h\mathrm{T}}$, as in a rare medium, but $\epsilon^{-h(\mathrm{T}+\kappa\mathrm{T}_r)}$.

4. To test that suggestion, consider the case of an infinite vertical column of gas subject to a constant vertical force f. We have, if s be the height above a fixed horizontal plane, $dp/ds = -\mathrm{M}f\rho$.

Assuming for the moment the whole energy to be that of relative motion T_r, that gives

$$\tfrac{2}{3}\frac{d}{ds}(\overline{1+\kappa}\,\rho\mathrm{T}_r) = -\mathrm{M}f\rho.$$

Now κ contains ρ as a factor. If we make T_r constant, as in the rare medium, the equation is impracticable. But make $\overline{1+\kappa}\mathrm{T}_r$ constant $= 3/2h$, and we get the usual equation $\rho = \rho_0\epsilon^{-h\mathrm{M}fs}$, ρ_0 being the value of ρ when $s = 0$.

5, 6, 7, 8. Now consider N spheres crossing the plane $s = 0$, with u for vertical component of velocity. Of these some will undergo collision before reaching ds. But an equal number will be substituted for them with the same vertical velocity, but with a small average advance in position in direction s, owing to the finite diameter c. It is shown that on average of the N spheres this advance is κds, and, therefore, the class of N spheres, original or substituted, will at the end of the time ds/u be at the height, not ds, but on average $(1+\kappa)ds$. But their loss of kinetic energy by the action of the force f is only $\mathrm{M}fds$. And, therefore, the loss due to the height ds is, allowing for substitutions, $\mathrm{M}fds/1+\kappa$.

9, 10. Hence we find that the assumption $\overline{1+\kappa}\mathrm{T}_r = 3/2h$ satisfies all the conditions of equilibrium in exactly the same way as in the rare medium $\mathrm{T}_r = 3/2h$ satisfies them.

11. The result can now be generalised by introducing stream motion, the energy of which is T_s, as well as that of relative motion T_r, and we find that $\mathrm{T}+\kappa\mathrm{T}_r$ must be constant throughout the column. Say, now, $\mathrm{T}+\kappa\mathrm{T}_r = 3/2h$.

12, 13. I have given elsewhere ('Science Progress,' November,

1894) reasons for assuming as the law of distribution of velocities among n spheres the expression

$$\epsilon^{-(a_1u_1^2+b_{12}u_1u_2+a_2u_2^2+b_{13}u_1u_3+b_{23}u_2u_3+\&c.)}$$

in which the coefficients have yet to be determined.

14, 15. The "a" coefficients must be all positive, the "b" coefficients all negative; and the b coefficients express the fact that the pairs of velocities to which they relate are not independent; and the b's, being negative, express the fact that the two velocities are more likely to be of the same than of opposite signs, so that there will be on the average of any group of contiguous spheres a greater common or stream motion than there would be were the velocities all independent.

16. The coefficients b must generally diminish as the distance between the two spheres to which they relate increases, becoming evanescent when that distance is great enough.

17. If the chance for a group of n spheres be of the form $C\epsilon^{-\lambda Q_n}$, and for a group of $n-1$ spheres, part of the n spheres, $C\epsilon^{-\lambda Q_{n-1}}$, Q_n and Q_{n-1} must be connected by the relation

$$\iiint_{-\infty}^{\infty} \epsilon^{-hQ_n} du_n dv_n dw_n = \epsilon^{-hQ_{n-1}}.$$

If we effect the integration for one variable we find, if

$$Q_n = a_1u_1^2+b_{12}u_1u_2+a_2u_2^2+\&c.$$

$$Q_{n-1} = a'_1u_1^2+b'_{12}u_1u_2+a'_2u_2^2+\&c.,$$

in which

$$2a'_1 = 2a_1-\frac{b^2_{1n}}{2a_n};$$

$$b'_{12} = b_{12}-\frac{b_{1n}b_{2n}}{2a_n}; \ \&c.$$

This shows that as n diminishes the a coefficients diminish, and since every b coefficient is negative the b's increase in absolute value, so that the ratios bb'/a or b^2/a increase. On the other hand, as n increases the a's increase, and the squares and products of the form b^2 or bb' diminish. Whence it is inferred that as n increases the function

$$Q_n = a_1u_1^2+b_{12}u_1u_2+\&c.$$

tends to assume a limiting form. This limiting form must be T when $\kappa = 0$, and must be such as to make $\overline{T}_r$ less than it would be were all the velocities independent. It may then be assumed to be $T+\kappa T_r$.

18, 19. Assuming the law to be $\epsilon^{-h(T+\kappa T_r)}$, we have

$$T+\kappa T_r = \left(1+\frac{n-1}{n}\kappa\right)\frac{u_1^2}{2}-\frac{\kappa}{n}u_1u_2-\frac{\kappa}{n}u_1u_3-\&c.$$

$$+\left(1+\frac{n-1}{n}\kappa\right)\frac{u_2^2}{2}-\&c.,$$

and, forming the determinant,

$$D = \left(1+\frac{n-1}{n}\kappa\right)-\frac{\kappa}{n}-\frac{\kappa}{n}-$$

$$-\frac{\kappa}{n}\left(1+\frac{n-1}{n}\kappa\right)-\frac{\kappa}{n}-\&c.$$

$$\cdots\cdots\cdots$$

With D_{11}, D_{12}, &c., for minors, we find

$$D = (1+\kappa)^n-n\frac{\kappa}{n}(1+\kappa)^{n-1} = (1+\kappa)^{n-1}.$$

$$D_{11} = D_{22} = \&c. = (1+\kappa)^{n-1}-\overline{n-1}\,\frac{\kappa}{n}(1+\kappa)^{n-2}$$

$$= \frac{n+\kappa}{n}(1+\kappa)^{n-2},$$

and therefore $$\overline{u_1^2} = \overline{u_2^2} = \&c.$$

$$= \frac{1}{hM}\frac{D_{11}}{D} = \frac{1}{hM}\cdot\frac{1}{n}\frac{n+\kappa}{1+\kappa}.$$

And since v^2 and w^2 have corresponding values, therefore

$$\overline{nT} = M\frac{3n}{2h}\overline{u_1^2} = \frac{3}{2h}\cdot\frac{n+\kappa}{1+\kappa}.$$

These results are easily obtained by considering the general determinant.

$$D = \begin{vmatrix} 2a & b & b & \ldots \\ b & 2a & b & \ldots \\ \ldots & \ldots & \ldots & \ldots \end{vmatrix}$$

It will be found for $n = 2$, $n = 3$, and thence by induction for all values of n, that $D = (2a-b)^n+nb(2a-b)^{n-1}$. Whence, replacing $2a$ by $1+\frac{n-1}{n}\kappa$, and b by $-\frac{\kappa}{n}$, we get the results above stated.

Again we find

$$\overline{n\mathrm{T}_r} = \frac{3}{2h}\frac{n-1}{1+\kappa},$$

and

$$\overline{n\mathrm{T}_s} = \overline{n\mathrm{T}} - \overline{n\mathrm{T}_r} = \frac{3}{2h},$$

and therefore $\frac{\mathrm{T}_s}{\mathrm{T}} = \frac{1+\kappa}{n+\kappa}$, which increases as κ increases, that is *ceteris paribus* as the diameter of the spheres increases.

Again the mean pressure per unit of area is $p = \frac{2}{3}(1+\kappa)\rho\overline{\mathrm{T}_r}$, which is independent of c. For a system of material points $p = \frac{2}{3}\rho\mathrm{T}_r$, that is $\frac{2}{3}(1+\kappa)\rho\mathrm{T}_r$, since in this case $\kappa = 0$. As the spheres increase in diameter with $(1+\kappa)\mathrm{T}_r$ constant, p remains constant.

The number of collisions per unit of volume and time varies as $c^2\sqrt{\overline{\mathrm{T}_r}}$, that is, as $\frac{\kappa^{\frac{2}{3}}}{\sqrt{1+\kappa}}$, and is, therefore, less than it would be if, with the same diameter, the spheres had velocities independent of each other.

20. It follows from the fact that p is independent of κ, that local variations of density, that is of κ, involve, on the whole, no expenditure of work, and will, in fact, come into being.

21. The effect of collisions between the spheres is now considered directly, to show how we obtain the known results that collisions between members of a group of spheres tend to reduce the group to the "special state" in which T_r is constant throughout the group. Let the component velocities of two spheres be $x_1y_1z_1$ $x_2y_2z_2$ before collision and $x'_1y'_1z'_1$ $x'_2y'_2z'_2$ after collision. Then, if the two are members of a group and the chance that the members of the group shall have assigned velocities is $c\epsilon^{-h\mathrm{Q}}$, in which

$$\mathrm{Q} = ax_1^2 + bx_1x_2 + ax_2^2 + \&c.,$$

the a coefficients being all alike and the b's all alike, we find that the chance for the velocities after the collision is $c\epsilon^{-h\mathrm{Q}'}$, in which Q' is the same function of $x'_1x'_2$, &c., that Q is of x_1x_2, &c. This shows that the distribution is not disturbed by collisions if all the a's are alike and all the b's alike. The group is in the special state.

22. But if the a's differ from each other or the b's differ from each other, it is shown that collisions tend to reduce them to equality, a with a and b with b; that is to reduce the group to the special state.

23. Boltzmann's minimum function tends to diminish by collisions, finally becoming constant for any group of contiguous spheres, when T_r becomes uniform throughout the group. On the other hand, as the group becomes too large, the spheres composing it develop an opposite tendency to split up into smaller groups, each with some

small stream motion relative to the others, and so to diminish the mean pressure and the number of collisions per unit of time. The actual state of the medium is a compromise between the two opposite tendencies.

Presents, February 7, 1895.

Transactions.

Basel:—Naturforschende Gesellschaft. Verhandlungen. Band X. Heft 2. 8vo. *Basel* 1894. The Society.

Batavia:—Bataviaasch Genootschap van Kunsten en Wetenschappen. Notulen van de Algemeene en Bestuursvergaderingen. Deel XXXI. Aflevering 3, en 4. Deel XXXII. Aflevering 1, en 2. 8vo. *Batavia* 1893–94; Catalogus der Ethnologische Verzameling. Vierde Druk. Supplement. 8vo. *Batavia* 1894; Nederlandsch-Indisch Plakaatboek 1602—1811. Twaalfde Deel, 1795—1799. 8vo. *Batavia* 1894; Tijdschrift voor Indische Taal, Land en Volkenkunde. Deel XXXVII. Aflevering 1—5. Deel XXXVIII. Aflevering 1 en 2. 8vo. *Batavia* 1893–94; Verhandelingen. Deel XLVII. 2^e Stuk. Deel XLVIII. 1ste Stuk. 8vo. *Batavia* 1893. The Academy.

Berlin:—Deutsche Chemische Gesellschaft. Berichte. 1894. Nos. 10—18. 8vo. *Berlin.* The Society.

Brussels:—Académie Royale de Médecine. Bulletin. Tome VIII. Nos. 5—10. 8vo. *Bruxelles* 1894. The Academy.

Académie Royale des Sciences. Bulletin. Tome XXVII. Nos. 5—11. 8vo. *Bruxelles* 1894. The Academy.

Florence:—Biblioteca Nazionale Centrale. Bollettino. 1894. Giugno-Dicembre. 8vo. *Firenze.* The Library.

London:—Chemical Society. Journal. July—December, 1894. 8vo. *London.* Proceedings. Nos. 140—146. 8vo. *London* 1894. The Society.

Geological Society. Abstract of the Proceedings. Nos. 628—634. 8vo. *London* 1894; Quarterly Journal. Vol. LI. Part I. 8vo. *London* 1895. The Society.

Institution of Civil Engineers. Abstract of Proceedings. Session 1894–95. Nos. 1—4. 8vo. *London.* The Institution.

Institution of Electrical Engineers. Journal. Vol. XXIII. Nos. 112, 113. 8vo. *London* 1894. The Institution.

Pharmaceutical Society of Great Britain. Pharmaceutical Journal and Transactions. July—December, 1894. 8vo. *London.* The Society.

Transactions (*continued*).

Royal Astronomical Society. Monthly Notices. Vol. LIV. Nos. 7—9. Vol. LV. Nos. 1—2. 8vo. *London* 1894. The Society.

Royal Geographical Society. The Geographical Journal. July—December, 1894. 8vo. *London.* The Society.

Royal Institution of Great Britain. Reports of the Weekly Evening Meetings. April—June, 1894. 8vo. *London.* The Institution.

Royal Photographic Society. Journal and Transactions. Vol. XIX. No. 5. 8vo. *London* 1895. The Society.

Society of Arts. Journal. July—December, 1894. 8vo. *London.* The Society.

Society of Biblical Archæology. Proceedings. Vol. XVI. Part 10. 8vo. *London* 1894. The Society.

Society of Chemical Industry. Journal. Vol. XIII. Nos. 5—12. 8vo. *London* 1894. The Society.

Munich:—Akademie der Wissenschaften. Abhandlungen. Math.-Phys. Classe. Band XVIII. Abth. 3. 4to. *München* 1895; Ueber die Bedeutung wissenschaftlicher Ballonfahrten: Festrede. Von. L. Sohncke. 4to. *München* 1894. The Academy.

New York:—American Geographical Society. Bulletin. Vol. XXVI. No. 4. Part 1. 8vo. *New York* 1894. The Society.

Paris:—Académie des Sciences. Comptes Rendus. Juillet—Décembre, 1894. 4to. *Paris.* The Academy.

École Normale Supérieure. Annales Scientifiques. Série 3. Tome XI. Supplément. 4to. *Paris* 1894. The School.

Société de Biologie. Comptes Rendus. Juillet—Décembre. 1894. 8vo. *Paris.* The Society.

Société d'Encouragement pour l'Industrie Nationale. Bulletin. Mai—Décembre, 1894. 4to. *Paris;* Compte Rendu des Séances. 1894. Nos. 11—18. 8vo. *Paris.* The Society.

Société de Géographie. Comptes Rendus des Séances. 1894. Nos. 13—19. 8vo. *Paris.* The Society.

Société Française de Physique. Bulletin Bimensuel. 1895. 8vo. *Paris.* The Society.

Société Géologique. Compte Rendu des Séances. 1894. Nos. 13—18. 8vo. [*Paris.*] The Society.

Société Philomathique. Compte Rendu des Séances. 1894. Nos. 15—19. 8vo. *Paris.* The Society.

Philadelphia:—Academy of Natural Sciences. Proceedings. 1894. Pp. 89—320. 8vo. *Philadelphia.* The Academy.

Franklin Institute. Journal. July—December, 1894. 8vo. *Philadelphia.* The Institute.

Transactions (*continued*).

Pisa:—Società Toscana di Scienze Naturali. Atti. Processi Verbali. Vol. IX. Pp. 133—192. 8vo. [*Pisa*] 1894. The Society.

Rome:—Reale Accademia dei Lincei. Rendiconti. Serie 5. Vol. III. Semestre 1. Fasc. 9—12. Semestre 2. Fasc. 1—12. 8vo. *Roma* 1894. The Academy.

Santiago:—Sociedad Nacional de Mineria. Boletin. Año XI. No. 72. 4to. *Santiago de Chile* 1894. The Society.

Stockholm:—Kongl. Vetenskaps-Akademie. Öfversigt. Årg. LI. No. 9. 8vo. *Stockholm* 1894. The Academy.

Sydney:—Linnean Society of New South Wales. Abstract of Proceedings. May—October, 1894. 8vo. *Sydney*. The Society.

Tokio:—College of Science. Imperial University. Journal. Vol. VII. Parts 2, 3. 4to. *Tōkyō* 1894. The University.

Turin:—R. Accademia delle Scienze. Atti. Vol. XXX. Disp. 1—4. 8vo. *Torino* 1894. The Academy.

Vienna:—Kais. Akademie der Wissenschaften. Anzeiger. Jahrg. 1894. Nos. 16—27. 8vo. *Wien*. The Academy.

Observations and Reports.

Calcutta:—Meteorological Department of the Government of India. Indian Meteorological Memoirs. Vol. V. Parts 4, 5. Folio. *Calcutta* 1894; Instructions to Observers of the India Meteorological Department. 8vo. *Calcutta* 1894. The Department.

California:—State Mining Bureau. Twelfth Report of the State Mineralogist. (Second Biennial.) Two years ending September 15, 1894. 8vo. *Sacramento* 1894. The Bureau.

Dublin:—General Register Office. Weekly Return of Births and Deaths. July—December, 1894. 8vo. *Dublin*. The Office.

Finland:—Geologiska Undersökning. Beskrifning till Kartbladets. Nos. 25, 26 [with two corresponding Maps]. 8vo. *Kuopio* 1894. The Commission.

Kiel:—Ministerial Kommission zur Untersuchung der deutschen Meere. Ergebnisse der Beobachtungsstationen. Jahrgang 1893. Hefte I—VI. 4to. *Kiel* 1894; Wissenschaftliche Meeresuntersuchungen. Neue Folge. Erster Band. Heft 1. Folio. *Kiel* 1894. The Commission.

Leeds:—Yorkshire College. 20th Annual Report. 1893–94. 8vo. *Leeds* 1894. The College.

Madrid:—Comisión del Mapa Geológico. Mapa Geológico de Espãna. [1 sheet.] *Madrid* [1895]. The Commission.

Observations and Reports (*continued*).

Washington:—U.S. Coast and Geodetic Survey. Report of the Superintendent. 1892. Part II. 8vo. *Washington* 1894.
The Survey.

U.S. Patent Office. Official Gazette. Vol. LXVII. No. 9—Vol. LXIX. No. 13. 8vo. *Washington* 1894. The Office.

Wellington, N.Z.:—Registrar-General's Office. New Zealand Statistics for 1893. Folio. *Wellington, N.Z.* 1894.
Registrar-General.

Journals.

American Chemical Journal. Vol. XVI. Nos. 5—8. 8vo. *Baltimore.* The Editor.

American Journal of Mathematics. Vol. XVI. Nos. 3, 4. 4to. *Baltimore* 1894. The Editors.

American Journal of Science. July—December, 1894. 8vo. *New Haven.* The Editors.

Analyst. July—December, 1894. 8vo. *London.*
Society of Public Analysts.

Annalen der Physik und Chemie. 1894. Nos. 7—12. 8vo. *Leipzig;* Beiblätter. 1894. Nos. 6—12. 8vo. *Leipzig.*
The Editors.

Annales des Mines. 1894. Livr. 5—12. 8vo. *Paris.*
École des Mines, Paris.

Annales des Ponts et Chaussées. 1894. Cahier 5—12. 8vo. *Paris.* Ministère des Travaux Publics, Paris.

Asclepiad. Vol. XI. No. 42. 8vo. *London* 1895.
Sir B. W. Richardson, F.R.S.

Astronomical Journal. Vol. XIV. Nos. 7—20. 4to. *Boston (Mass.)* 1894. Smithsonian Institution.

Astronomie (L'). Juillet—Décembre, 1894. 8vo. *Paris.*
The Editor.

Athenæum. July—December, 1894. 4to. *London.*
The Editor.

Builder. July—December, 1894. Folio. *London.*
The Editor.

Chemical News. July—December, 1894. 8vo. *London.*
Mr. W. Crookes, F.R.S.

Cosmos. Juillet—Décembre, 1894. 4to. *Paris.* The Editor.

Éclairage Électrique (L'). Tome I. Nos. 4—15. 4to. *Paris* 1894. The Publisher.

Educational Times. July—December, 1894. 4to. *London.*
College of Preceptors.

Electrical Engineer. July—December, 1894. Folio. *London.*
The Editor.

Journals (*continued*).

Electrical Review. July—December, 1894. Folio. *London.*
The Editor.

Electrician. July—December, 1894. Folio. *London.*
The Editor.

Électricien (L'). Juillet—Décembre, 1894. Folio. *Paris.*
The Editor.

Horological Journal. Vol. XXXVII. No. 438. 8vo. *London* 1895.
British Horological Institute.

Industries and Iron. July—December, 1894. 4to. *London.*
The Editor.

Meteorologische Zeitschrift. 1894. Heft 6—12. Sm. Folio. *Wien.*
Oesterreichische Gesellschaft für Meteorologie.

Morskoi Sbornik. 1894. Nos. 3—11. [*Russian.*] 8vo. *St. Petersburg.* Compass Observatory, Cronstadt.

Nature. July—December, 1894. Roy. 8vo. *London.*
The Editor.

Nature Notes. Vol. VI. No. 62. 8vo. *London* 1895.
The Selborne Society.

New York Medical Journal. July—December, 1894. 4to. *New York.* The Editor.

Observatory. July—December, 1894. 8vo. *London.*
The Editors.

Physical Review. Vol. II. No. 4. 8vo. *New York* 1895.
The Editors.

Revue Générale des Sciences. Juillet—Décembre, 1894. 8vo. *Paris.* The Editor.

Revue Scientifique. Juillet—Décembre, 1894. 4to. *Paris.*
The Editor.

Stazioni Sperimentali Agrarie Italiane. Vol. XXVIII. Fasc. 1. 8vo. *Modena* 1895. R. Stazione Agraria, Modena.

Symons's Monthly Meteorological Magazine. July—December, 1894. 8vo. *London.* Mr. G. J. Symons, F.R.S.

Abney (Capt. W. de W.), F.R.S. Colour Vision; being the Tyndall Lectures delivered in 1894 at the Royal Institution. 8vo. *London* 1895. The Author.

Albert 1^{er} (Prince de Monaco) Sur les Premières Campagnes Scientifiques de la Princesse Alice. 4to. *Paris* 1895.
H.H. The Prince of Monaco.

Alfārābīs Abhandlung der Musterstaat; aus Londoner und Oxforder Handschriften herausg. von Dr. Friedrich Dieterici. 8vo. *Leiden* 1895. The Editor.

Bickerton (A. W.) Copies of letters sent to 'Nature,' on "Partial Impact." 8vo. *Christchurch, N.Z.*, 1879; Some recent evidence in favour of Impact. 8vo. *Wellington, N.Z.* 1894.
The Author.

Buchanan (J. Y.), F.R.S. Sur la densité et l'alcalinité des eaux de l'Atlantique et de la Méditerranée. 4to. [*Paris* 1895.]
H.H. The Prince of Monaco.

McClintock (E.) Theorems in the Calculus of Enlargement: a method for calculating simultaneously all the roots of an equation. 4to. [*Baltimore*] 1894.
The Author.

Roscoe (Sir H. E.) & Classen (A.) Roscoe-Schorlemmer's Lehrbuch der Anorganischen Chemie. Erster Band. Dritte Auflage. Erste Abth. 8vo. *Braunschweig* 1895.
Sir Henry Roscoe, F.R.S.

Vogel (H. C.) Neuere Untersuchungen über die Spectra der Planeten. 8vo. *Berlin* 1895.
The Author.

February 14, 1895.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

The Right. Hon. Horace, Lord Davey, a Member of Her Majesty's Most Honourable Privy Council, was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On some Considerations showing that Maxwell's Theorem of the Equal Partition of Energy among the Degrees of Freedom of Atoms is not inconsistent with the various Internal Movements exhibited by the Spectra of Gases." By Professor G. F. FITZGERALD, F.R.S. Received February 7, 1895.

It has been generally held that a sufficient freedom of internal motion in an atom to explain the spectra of gases proved that the theorem as to equal partition of energy among all degrees of freedom could not hold, and various suggestions have been made as to why the *proof*, as given by Maxwell, Boltzmann, and others, fails in this case. Professor Schuster has suggested that the numerous lines need not involve the same number of degrees of freedom, as it is possible that there may be connections between them such that one or two co-ordinates would define a motion which when analysed into its Fourier components, as is done by a grating or prism, would produce a very complex system of lines. However, even one degree of internal freedom would interfere very seriously with the observed value of the ratio of specific heats, and the object of this letter is to explain how this difficulty may be surmounted without supposing that the theorem as to equal partition of energy is untrue, for it is not by any means disproved because a certain form of proof fails in certain cases.

It has been long held that the motion of the electrons on neighbouring atoms is very much controlled by the ether between them. The wave-length of light is generally many times as great as the molecular distances, so that the ether is a practically rigid connector between neighbouring electrons. Suppose now, as a particular example, that 10^6 atoms are in this sense, and so far as the motion of electrons is concerned, within one another's control. In this case the motion of these 10^6 electrons might be defined by means of, say, three co-ordinates. Hence, if the atoms were spheres, there would be 3×10^6 degrees of freedom plus these three degrees defining the motions of all the electrons. Now, if the total energy be equally distributed among all these degrees of freedom, each atom will only have its share of the electromotions, and its energy of external motion will only be diminished by 3×10^{-6}th part owing to the existence of the internal motion of its electrons. I need hardly say that our methods of calorimetry are by no means sufficiently delicate to detect anything of this kind. There might be a thousand such internal degrees of freedom, and yet the ratio of specific heats would agree with observation.

There is some analogy between this suggestion and the case of a sphere moving in a liquid. The presence of the liquid, although apparently endowed with an infinite number of degrees of freedom, does not really increase the degrees of freedom at all, because its motion is entirely defined by the motion of the sphere. In a somewhat similar manner, I would suggest that the presence of the million electrons does not sensibly increase the degrees of freedom of motion of the million atoms, as all their motions may be defined in terms of the motion of a few of them. That the ether would so control the motions of electrons seems almost certain from what we know of the rapidity with which electromagnetic actions are transmitted by it, showing how completely it behaves in respect of them as a system of rigid connections.

II. "Contributions to the Chemistry of Chlorophyll. No. VI." By EDWARD SCHUNCK, F.R.S., and LEON MARCHLEWSKI, Ph.D. Received January 25, 1895.

The preceding memoir on the chemistry of chlorophyll contained the results arrived at in the analysis of phyllotaonin and its derivatives. In the present communication it is proposed to give further details regarding the properties and composition of alkachlorophyll, the mother substance of phyllotaonin, on which will follow an account of some products of decomposition of the latter. Some experiments will then be described showing the connection between phylloxanthin and phyllocyanin, and proving that the former may by a simple process be converted into the latter.

In order to determine the composition of alkachlorophyll, it was necessary to prepare specimens of the substance purer than those previously obtained, which were only just sufficiently pure for a determination of its chief properties. For this purpose we employed, as in previous experiments, an extract of grass with boiling alcohol.

We also made use of a commercial product furnished by Merck of Darmstadt, under the name of "Chlorophyllum purissimum," which, on examination, turned out to be not exactly what its name would betoken, but a very impure alkachlorophyll.

In the case of grass, the first part of the process was the same as that described in the fourth memoir of this series, and the details may therefore be omitted here. The sodium salt of alkachlorophyll of this process was treated for some time with a mixture of equal parts of absolute alcohol and ether, in order to remove the fatty acid soaps present. The sodium salt was then decomposed in the manner previously described, and the free alkachlorophyll obtained having been dissolved in ether, the solution was mixed with ligroin.

The latter precipitated the alkachlorophyll, the fatty acids still present remaining dissolved. The solution in ether and precipitation with ligroin was twice repeated, and the resulting product was treated several times with small quantities of boiling ligroin.

The product supplied by Merck, after being treated for some time with alcoholic soda, was submitted to the same process as that just described.

The substance dried at 125° yielded on analysis the following results, the first two determinations being made with the product from grass, the third with that from Merck's "Chlorophyll."

I. 0·1875 gram substance gave 0·4813 gram CO_2 and 0·1101 gram H_2O.

0·1968 gram substance gave 19·7 c.c. of nitrogen at 23° and 745 mm. pressure.

II. 0·1744 gram substance gave 0·4478 gram CO_2 and 0·0974 gram H_2O.

0·2293 gram substance gave 23·3 c.c. of nitrogen at 24° and 764 mm. pressure.

III. 0·2145 gram substance gave 0·5500 gram CO_2 and 0·1242 gram H_2O.

0·1072 gram substance gave 10·70 c.c. of nitrogen at 21° and 757 mm. pressure.

These numbers correspond in 100 parts to—

	I.	II.	III.	Mean.
C........	70·00	70·00	69·93	69·97
H........	6·52	6·20	6·43	6·37
N........	11·03	11·43	11·31	11·27

There are two formulæ with which these numbers agree equally well, viz., $C_{30}H_{34}N_2O_4$ and $C_{52}H_{57}N_7O_7$, requiring respectively

	$C_{30}H_{34}N_2O_4$.	$C_{52}H_{57}N_7O_7$.
C	70·04	70·12
H........	6·61	6·39
N	10·89	10·99
O	12·46	12·50

Since phyllotaonin, with the formula $C_{40}H_{40}N_6O_6$, is formed by the action of acids on alkachlorophyll, it was necessary to adopt for the latter a formula with no less than 6 atoms of nitrogen. The second of the above formulæ meets this requirement.

The formation of phyllotaonin from alkachlorophyll by the action of acids, supposing the process to be one of simple hydrolysis, might be represented by the following equation:—

$$C_{52}H_{57}N_7O_7 + H_2O = C_{40}H_{40}N_6O_6 + C_{12}H_{19}NO_2.$$

That a small quantity of a product of a basic nature probably containing nitrogen is formed along with phyllotaonin, by the action of acids on alkachlorophyll, has already been mentioned in the fourth memoir of this series, but we have not been able to throw any more light on the nature of this product. That some substance containing nitrogen must be formed together with phyllotaonin in the decomposition, is also indicated by the equation just given, but whether the formula $C_{12}H_{19}NO_2$ represents one substance only, or a mixture of several, is doubtful.

The properties of alkachlorophyll have been already described, and need not therefore be referred to here. The action of alkalis on it at a high temperature may however be briefly described.

When a solution of alkachlorophyll in alcoholic potash is heated in a sealed tube for several hours at 190—200° it is decomposed; on opening the tube no pressure is perceived.

The contents, consisting partly of small brownish-red crystals, having been poured out, an excess of hydrochloric acid is added; this dissolves a part, yielding a purple solution.

The latter is filtered off from the undissolved brown mass, and after being nearly neutralised with soda, yields on agitation with ether a solution of a fine crimson colour. The ethereal solution after washing with water leaves on evaporation a quantity of small crystals. The substance so obtained is identical with the one formed by the same process from phyllotaonin and which will be described presently. It seems also to be essentially the same as that obtained by Tschirch in acting on alkachlorophyll with alkali, and called by him "phylloporphyric acid," but a comparison of the absorption spectrum of the latter as described by him with that of our substance shows that his product must have been impure. This is evident from the fact that the absorption spectrum of Tschirch's product shows, according to him, a band in the red about in the same position as band I of the chlorophyll spectrum, whereas this band though visible in solutions of our substance when the latter is not quite pure, disappears entirely in solutions of the pure substance.

The liquid filtered off from the products insoluble in hydrochloric acid of the process just described, contains in combination with the acid various volatile bases, chiefly ammonia. In one experiment the liquid, after being made alkaline with soda, was submitted to distillation, the vapour evolved being passed into dilute hydrochloric acid. The acid liquid after concentration yielded with platinic chloride a yellow precipitate, which was collected, treated with boiling alcohol, and dried.

0·1031 gram of the substance gave 0·0451 gram platinum = 43·74 per cent.; calculation for $(NH_4)_2PtCl_6$ requires 43·90 per cent.

The filtrate from the platinum ammonium chloride yielded, on evaporation, orange coloured octahedra, containing 38·7 per cent. of platinum. It may hence be inferred that along with ammonia some base of higher molecular weight is formed; the quantity is, however, very small.

Phylloporphyrin.

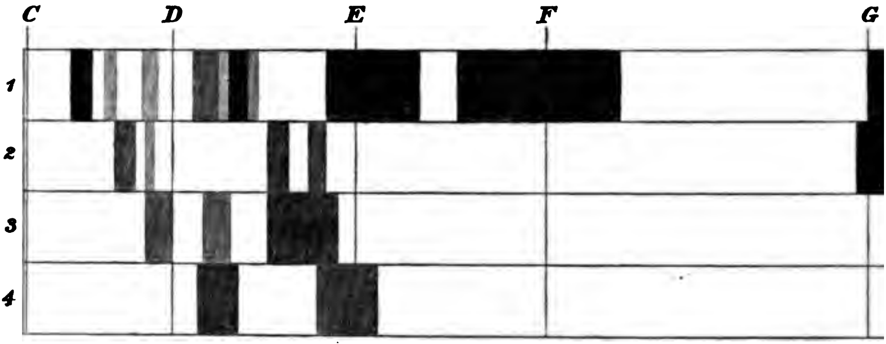
The action of caustic alkalis on chlorophyll and its derivatives has been previously investigated. Hoppe-Seyler obtained by acting on his chlorophyllan with alkali at 260—290° a substance giving purple solutions. On account of the double fluorescence of its solutions observed by him he gave it the name of "dichromatic acid." The discoverer states that it contains no nitrogen, the analysis of the barium salt leading for the acid to the formula $C_{20}H_{34}O_3$, and it is, he states, a very unstable body. The absorption spectrum of its solutions shows six bands in the relative positions indicated by him, Among the products of decomposition of dichromatic acid mention is made of a substance which in acid solutions shows only two bands; this substance Hoppe-Seyler calls phylloporphyrin. Another product of decomposition is obtained, according to him, on evaporating the ethereal solution. Sachsse obtained by fusion of his β-phæochlorophyll with soda a reddish-brown product dissolving in acids with a violet colour, and having the formula $C_{26}H_{33}N_3O_2$. Similar experiments made with phyllocyanin, which led to the formation of a purple crystallised substance, have already been described in the fourth memoir of this series. The product formed in all these cases being doubtless the same, we shall adopt for it the name phylloporphyrin, Hoppe-Seyler's body of the same name being, in our opinion, simply a compound with acid, not a product of decomposition. The substance has, in fact, a pronounced basic character, and shows in neutral solutions a spectrum differing entirely from that of its acid solutions.

Preparation and Properties of Phylloporphyrin.

The method of preparing the substance from phyllocyanin has previously been described. The method before given is, however, not an advantageous one, the yield being trifling, and the quantity of bye-products formed at the same time relatively large. We therefore adopted another process, avoiding the use of melted alkali in open vessels and employing alcoholic potash in sealed tubes. The material employed was phyllotaonin, which, being itself a product of decomposition of phyllocyanin with alkali at lower temperatures, and being more easily prepared in a state of purity than phyllocyanin, seemed better adapted than the latter for the preparation of phylloporphyrin. The process employed was as follows:—Ethyl phyllotaonin in crystals was treated with alcoholic potash in sealed tubes at 190° for several hours. On opening the tubes no pressure was observed. The brownish-red contents were poured out, mixed with water and an excess of hydrochloric acid, and then shaken up with ether. The ether acquired a fine purple colour, and left on evaporation a brownish residue in which dark purple crystals were discernible. The residue was treated with boiling alcohol, which dissolved part, leaving a quantity of brown matter undissolved. On adding to the filtrate an alcoholic solution of zinc acetate, and allowing it to stand, a red crystalline deposit—a zinc compound of phylloporpbyrin—was formed. This was collected and dissolved in boiling alcohol. The solution, after addition of a little hydrochloric acid, was mixed with much water and extracted with ether. The latter acquired a splendid crimson colour, and, having been washed with water, left on evaporation reddish-violet crystals, which were recrystallised from alcohol.

As thus prepared, phylloporphyrin appears in the form of lustrous crystals of a splendid reddish-violet colour, which under the microscope are seen to be short regular prisms. It is tolerably soluble in alcohol and ether, giving red fluorescent solutions, which, on the addition of a little acid, acquire a bluish tint. It dissolves in mineral acids as well as in glacial acetic acid. The solutions give up nothing when shaken up with ether, even when previously mixed with much water, showing that they contain compounds of the substance with the respective acids. When, however, the solutions are made alkaline and then again slightly acid, then ether, on agitation, takes up the substance. On adding nitric acid to the solution in acetic acid it becomes greenish, and, on heating, the colour disappears entirely. Phylloporphyrin possesses also acid properties, though these are not very pronounced. It is, indeed, quite insoluble in aqueous alkalis; but when caustic soda is added to its alcoholic solution a brown sodium compound separates, which yields up nothing to ether until the alkali has been removed by means of acetic acid. It yields, moreover, as before mentioned, a crystallised zinc salt, which is formed on adding zinc acetate to its alcoholic solution, when it crystallises out in small red needles; the compound is decomposed by hydrochloric acid, but it is scarcely changed by acetic acid. The zinc salt is of some importance in connection with the preparation of the pure substance, since by its means it becomes possible to separate it from other products of decomposition, more especially from the brown amorphous substances accompanying it.

As regards its absorption spectrum, phylloporphyrin is a very interesting substance. The ethereal solution shows seven bands, which are, for the most part, very sharply defined (see fig.); it is the first of the derivatives of chlorophyll, so far discovered, which shows in solution no band within the red, the first band being at the margin of and almost outside that part of the spectrum. The spectrum of the alcoholic solution is similar, but the second and third bands are not seen, and the fourth and fifth bands appear as one. The alcoholic solution to which hydrochloric acid has been added shows an absorption spectrum of three bands only, one close to line B, another near E, and a third, which is very faint, between the two. This spectrum should, we thought, have been the same as that of Hoppe-Seyler's phylloporphyrin, which we consider to be a compound of our phylloporphyrin with acid; that they are not quite identical is probably due to Hoppe-Seyler's product having been impure. That a solution of phylloporphyrin to which hydrochloric acid has been added really contains a compound with acid and not a product of decomposition, is evident from the fact that if the acid be neutralised with soda the solution loses its bluish tinge, and now shows the absorption spectrum of an alcoholic solution of the free substance. A solution of phylloporphyrin in concentrated sulphuric acid shows a spectrum with four bands. The spectrum of solutions of the zinc salt is peculiar. While the solutions of the alkaline salts show a spectrum differing very little from that of the substance itself, the zinc salt in solution shows only two bands (see fig.). On adding a little hydrochloric acid to the zinc salt solution and boiling, the spectrum of the hydrochloride makes its appearance, followed, after considerable dilution, by that of the substance itself.



EXPLANATION OF FIGURES.

Absorption Spectra of Phylloporphyrin and its Compounds.

1. Phylloporphyrin in ether.
2. The same in concentrated sulphuric acid.
3. The same in hydrochloric acid.
4. Zinc salt.

Phylloporphyrin, like most of the derivatives of chlorophyll hitherto examined, contains nitrogen, differing in this respect from the dichromatic acid of Hoppe-Seyler, which, according to him, is devoid of nitrogen.

The analysis of phylloporphyrin and of its zinc salt yielded the following results :—

0·1023 gram phylloporphyrin gave 0·2850 gram CO_2 and 0·0654 gram H_2O.

0·0920 gram gave 9·00 c.c. of nitrogen at 21° and 753 mm. pressure.

0·0848 gram of the zinc salt gave 0·2093 gram CO_2 and 0·0466 gram H_2O.

These numbers correspond in 100 parts to—

	Phylloporphyrin.	Zinc salt.
C	75·98	67·31
H	7·10	6·10
N	11·02	—

The formulæ $C_{32}H_{34}N_4O_2$ and $C_{32}H_{32}N_4O_2Zn$ require respectively—

	Phylloporphyrin.	Zinc salt.
C	75·89	67·46
H	6·73	5·63
N	11·06	—
O	6·32	—

Want of substance prevented our making further determinations in confirmation of those above given.

It is difficult at present to explain in a simple manner the formation of phylloporphyrin from phyllotaonin, the more so since phylloporphyrin is not the only product of decomposition due to the action of alkali, it being always accompanied by a relatively large quantity of brown amorphous substances as before mentioned. Ammonia, together with a small quantity of another base having a higher molecular weight, could also be detected in the acid liquid filtered off from the phylloporphyrin and the brown substances, just as in the case of alkachlorophyll; the presence of these bodies was discovered in the manner previously described.

Conversion of Phylloxanthin into Phyllocyanin.

Phylloxanthin and phyllocyanin being both products of decomposition of chlorophyll with acids, and having many properties in common, it seemed probable that a connection of an intimate character might be found to subsist between the two substances. Our experiments lead to the conclusion that this is, in fact, the case, and that phylloxanthin may by the prolonged action of acids be converted into phyllocyanin. This convertibility has frequently been suspected, but has never yet been satisfactorily proved.

The changes which take place in the spectrum of chlorophyll on the addition of acids to its solutions have long been known, they have been accurately described by Russell and Lapraik in their memoir entitled "A Spectroscopic Study of Chlorophyll." *

According to these observers, when a few drops of hydrochloric acid are added to an ethereal solution of chlorophyll, the colour of the latter changes from bright green to yellowish-green. At the same time the spectrum of the solution undergoes a marked change: bands

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* 'Chem. Soc. Trans.,' vol. 41, p. 334, 1882.

I and II appear more clearly defined in consequence of the clearing up of the space between them, while band III is shifted nearer the blue end and becomes paler, band IV, on the other hand, becoming much darker. After the solution has stood for a few days a fifth band nearer the blue end, and equal in intensity to the fourth, appears. The same changes take place, but more slowly, when tartaric acid is employed in place of hydrochloric acid. When acetic acid, however, is taken, the solution remains unchanged for some time; it then becomes yellowish-green and shows four bands as before, but the fifth band never makes its appearance however much the action may be prolonged. These changes in the spectrum of chlorophyll solutions on the addition of various acids may be explained by supposing that phylloxanthin is formed in the first instance, and is then, by the continued action of the acid, converted into phyllocyanin. The conversion though slow, on the whole proceeds more rapidly at the commencement than towards the end of the process, and is evidently due to hydrolysis. Similar phenomena are observed in the case of melitose, the metamorphosis of which also passes through two stages, being first converted by the action of emulsin into melibiose, which then by hydrolysis with acid yields galactose and fructose.

The phylloxanthin employed in the experiments about to be described was obtained in accordance with the directions given in the fourth memoir of this series. We have nothing to add to the account there given of the properties of phylloxanthin, except that when quite pure its solutions show only four absorption bands, not five as there stated, a fifth band, when it appears, indicating the presence of phyllocyanin.

Into an ethereal solution of phylloxanthin free from phyllocyanin, a current of dry hydrochloric acid gas was passed, the solution being kept cool, until quite saturated, and it was then shaken up with concentrated hydrochloric acid. The latter gradually acquired a bluish-green colour, which became darker on standing. The two liquids having remained in contact for some time were separated, and the lower acid one was mixed with several times its volume of water, which gave an almost black flocculent precipitate. The whole was then shaken up with ether, in which the precipitate dissolved, yielding a solution which, after washing with water, showed the olive-green colour peculiar to solutions of phyllocyanin as well as the five absorption bands belonging to the latter in their usual position and with their usual relative intensities. That a formation of phyllocyanin had actually taken place was proved by the following experiments. The ethereal solution of the product obtained was evaporated. Part of the residue left was dissolved in caustic soda; the solution, after standing for some time, was supersaturated with acetic acid, and then shaken up with ether; the ethereal solution was evaporated, and the residue was dissolved in boiling glacial acetic acid; the solution on standing deposited small crystalline grains, the ethereal solution of which showed the absorption spectrum of acetylphyllotaonin. Another portion of the residue left on evaporation of the ethereal solution was dissolved in boiling alcoholic potash; the solution, after filtration and addition of water, was acidified with acetic acid and then shaken up with ether; an ethereal solution was obtained showing the characteristic absorption spectrum of phyllotaonin.

The experiments just described proving the convertibility of phylloxanthin into phyllocyanin, tend to support the view held by some observers that only one chlorophyll really exists, and that there are not two or more chlorophylls as has sometimes been maintained. Under the action of acids, chlorophyll yields phylloxanthin, which is then converted into phyllocyanin. The latter when treated with alkalis gives phyllotaonin, and this, by the more energetic action of alkalis at a higher temperature, yields phylloporphyrin. The progressive increase in the number of absorption bands in passing through this series, and the gradual shifting of the bands from the red to the blue end of the spectrum, are points worthy of notice.

III. "On the Ratio of the Specific Heats of some Compound Gases." By J. W. CAPSTICK, D.Sc., M.A., Fellow of Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received January 25, 1895.

(Abstract.)

The experiments described are a continuation of those of which an account was given in the 'Philosophical Transactions,' vol. 185, p. 1.

The apparatus and method of procedure were the same as were described in the former paper. Kundt's dust-figure method was used for finding the velocity of sound in the gas, and the ratio of the specific heats was calculated by means of the formula

$$\gamma = 1 \cdot 408 \times \rho \times \left(\frac{l}{l'}\right) \left\{ 1 + \frac{1}{p} \frac{d}{dv} (pv) \right\},$$

the last factor being added to the ordinary formula in order to correct for the deviation of the gas from Boyle's law.

The densities and isothermal curves were determined experimentally.

The results are as follows:—

Name.	Formula.	γ.
Methylene chloride	CH_2Cl_2	1·219
Chloroform	$CHCl_3$	1·154
Carbon tetrachloride	CCl_4	1·130
Ethylene chloride	$C_2H_4Cl_2$	1·137
Ethylidene chloride	$C_2H_4Cl_2$	1·134
Ethylene	C_2H_4	1·264
Vinyl bromide	C_2H_3Br	1·198
Allyl chloride	C_3H_5Cl	1·137
Allyl bromide	C_3H_5Br	1·145
Ethyl formate	$HCOOC_2H_5$	1·124
Methyl acetate	CH_3COOCH_3	1·137
Sulphuretted hydrogen	SH_2	1·340
Carbon dioxide	CO_2	1·308
Carbon disulphide	CS_2	1·239
Silicon tetrachloride	CCl_4	1·129

From these and the results given in the former paper it is shown that corresponding halogen derivatives of the same hydrocarbon have the same γ, the statement being no longer restricted to the monohalogen derivatives of the paraffins.

The equality of the γ's of the two dichlorethanes, and of methyl acetate and ethyl formate, shows that the previously investigated case of the two propyl chlorides does not stand alone, but that it is probably true that isomeric bodies in general have the same γ.

From the results for the substitution products of methane and ethane with more than one halogen in the molecule it is shown that, whether the first chlorine introduced into the molecule alters the value of γ or not, each chlorine after the first causes a fall in γ.

$SiCl_4$ has the same γ as CCl_4. SH_2 has nearly the same γ as some observers have found for OH_2, but CO_2 and CS_2 differ widely, whence it follows that, just as in the case of hydrogen and chlorine, the possibility of interchange of oxygen and sulphur without altering γ does not extend to the substitution of two atoms of sulphur for two of oxygen.

In the paper a proof is given of the formula

$$\beta+1 = \frac{\frac{2}{3}+\frac{1}{p}\frac{d}{dv}(pv)}{\gamma-1}$$

for calculating β, the ratio of the rates of increase of intramolecular energy and translational energy of the molecule on a rise of temperature, and the constant β is given for the gases investigated.

It is shown that $\frac{\beta+1}{n}$ is constant for the paraffins and their monohalogen derivatives, whence it follows that for these the ratio of the increase of mean total energy to the increase of kinetic energy of translation of the molecule is proportional to the number of atoms in the molecule.

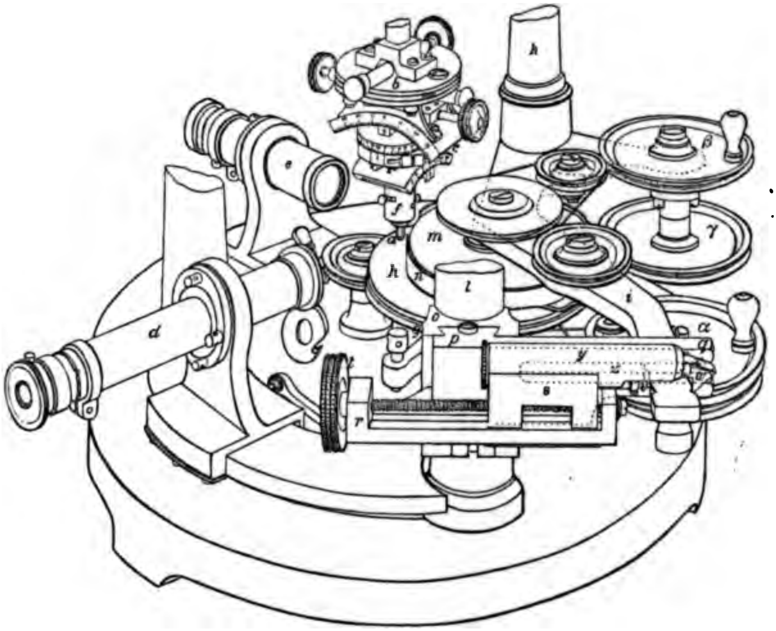
IV. "An Instrument for Cutting, Grinding, and Polishing Section-plates and Prisms of Mineral or other Crystals Accurately in the Desired Directions." By A. E. TUTTON, Assoc. R.C.S., Demonstrator of Chemistry at the Royal College of Science, South Kensington. Communicated by Professor JUDD, F.R.S. Received November 28, 1894.

(Abstract.)

In a recent communication ('Phil. Trans.,' 1894, Series A, p. 887; 'Roy. Soc. Proc.,' vol. 55, p. 108) the author described an instrument for grinding accurately orientated section-plates and prisms of crystals of artificial preparations. The success of that instrument is so complete that another instrument has been devised and constructed, upon similar principles, but with the necessary modifications and additions, to enable equally accurately orientated plates or prisms to be prepared from the relatively harder crystals of natural minerals.

The new instrument is constructed upon a scale one-fifth larger than the former one as regards such parts as are fundamentally similar, to confer greater strength. The main innovations are those of a cutting apparatus, capable of ready removal, in order not to impede goniometrical, grinding, and polishing operations, and a much larger grinding table fitted with a particularly convenient mode of attachment for any one of nine interchangeable grinding and polishing laps, suitable for use with crystals of every degree of hardness. The accompanying illustration represents the lower portion of the instrument, drawn from a point sufficiently high above the base to most clearly exhibit the arrangement of the cutting apparatus. The instrument is not intended to replace the one previously described, which is fully adapted to all the uses of chemical crystallographers, and the cost of which is only two-thirds that of the one now described. It is intended especially for the use of mineralogists, but, naturally, will likewise serve all the purposes of the smaller instrument.

The mode of supporting the outer fixed cone within which the movable axes rotate, the construction of the circle and its axis and fine adjustment, and of the gun-metal axis and its counterpoising levers designed for controlling the pressure between crystal and lap, as also of the inner steel axis from which are suspended the crystal *a*



Instrument for Cutting, Grinding, and Polishing Section-plates and Prisms of Mineral or other Crystals.

and its centering (*b*) and adjusting (*c*) movements, are similar in principle to the corresponding arrangements in the smaller instrument, although many details are altered for the sake of greater rigidity. The same likewise applies to the goniometrical telescope *d* and collimator *e* and their mode of support.

The crystal holders, in addition to being provided with cross-grooved surfaces for the reception of the cementing wax, which should be the hardest variety used by opticians, are further provided with protecting rims, so that the wax is firmly held in a shallow cup. Three ordinary holders are supplied, of $\frac{5}{16}$, $\frac{5}{8}$, and $\frac{7}{8}$ in. diameter respectively. Two holders, of $\frac{5}{16}$ and $\frac{1}{2}$ in. diameter, are also furnished, which permit of a certain amount of rotation of the crystal after fixation of the holder in its socket, as described in the former communication, and one of which is shown in position at *f* in the illustration. Also there is the special holder for use in grinding a second surface parallel to the one first prepared, and it is provided with two lower caps pierced by apertures of $\frac{9}{16}$ and $\frac{5}{16}$ in. respectively, together with a dozen glass discs ground to fit each.

The grinding apparatus resembles that of the smaller instrument, as far as the driving gear and the mode of supporting the grinding table are concerned. The table itself is much larger however, the laps being 4½ ins. in diameter, and it is differently constructed, in order to permit of rapid change from one lap to another. The table is provided with a permanent rigid gun-metal top *g*, slightly larger than the laps, upon which the latter are laid. The laps carry below three cylindrical projections, which pass through corresponding holes in the table, and they may be locked in this position by a plate carried beneath the table, and which is rotatable for a sufficient amount by a rack and pinion actuated by a short lever; the plate is pierced by tapering slots, wide enough at one end to allow the projections to fall through or be removed, but narrowed at the other so as to gear in niches cut in the projections. The lap *h* is placed in position, or lifted out when it is desired to remove it, while the lever is rotated outwards radially to the axis of the table; upon rotating the lever inwards again, as far as a stop permits it to go under the table, the lap, if in position, will be firmly locked to the latter.

Four metallic laps are provided; one of iron, for rough grinding with coarse emery and brick oil or water; two of gun-metal and hard white metal respectively, for fine grinding with flour emery; and another of pewter, for polishing with rottenstone and water. A polishing lap of hard felt, for use with putty powder and water, and a lap of boxwood, either for grinding soft mineral crystals or for polishing, are likewise supplied. There are also three glass laps for use with artificial crystals, one moderately ground for rough grinding, the second extremely finely ground, and another of plain plate glass, the last two for polishing, and all three to be lubricated with oil. The final polisher of ordinary plate glass furnishes admirably polished surfaces, and best of all when the amount of oil is extremely small.

The cutting apparatus is carried at a suitable height above the level of the grinding table upon a rigid horizontal arm pivoted upon the back pillar *k* of the instrument, so that it can be completely rotated out of the way during grinding operations, and further supported when required for use upon an adjunct of the right front pillar *l*. It consists of a 4-in. disc of soft iron *m*, supplied with diamond edge, and intended to be lubricated with brick oil, supported truly parallel to the grinding table between two stout broad washers, carried by the almost frictionless axle, and driven by an independent driving gear; the latter is somewhat similarly arranged to that of the grinding apparatus, in order that the pressure of the band round the pulley which turns with the axle of the cutter shall be equal on each side to minimise friction, and is entirely carried upon the arm. The latter is bent inwards towards the crystal, so that the cutting edge may be conveniently approached.

Three interchangeable cutting discs are supplied, one of the usual thickness of a lapidary's cutter, and two somewhat thicker, which are preferable on account of their greater rigidity; their edges are carefully squared and charged with diamond dust in the usual manner. In order to protect the instrument from particles projected during cutting, the disc is surrounded, except in the vicinity of the crystal, with a readily removable guard *n*. A second larger guard is also supplied in order to afford more complete protection to the optical tubes; it consists of a thin metal disc considerably larger than the grinding table, upon which it is intended to rest, and fitted round one-half of its circumference with a rim $1\frac{3}{4}$ in. high, sufficient to protect the telescope and collimator, while not interfering with the freedom of movement of the arm of the cutting apparatus. The support upon the front pillar is removable when the cutter is not in use, so as to leave room for grinding; the pillar carries in front at the convenient height a rectangular thickening *o*, out of which a vertical dove-tailed recess is cut, within which a corresponding dovetail *p*, forming the back portion of the main casting of the attachment, is capable of sliding until arrested at the proper height, corresponding to the perfectly horizontal position of the cutter and its arm, by a stop.

This attachment includes not only the supporting fork *q* for the arm, but also the apparatus for directing and controlling the cutting. The latter consists of a horizontal traversing bed *r* and slider *s*, manipulated by the large milled head *t* of the traversing screw; the arm is attached to the slider by being gripped between a small spring piston *v* and a hinged wedge-shaped hook *w*, the latter being so shaped in order that it may be pushed out of the way while the arm passes it, after which a spring *x* causes it to fall down behind the arm. This gripping arrangement is carried by a much larger piston, retained by a strong spring in a cylinder *y* fixed to the slider. The possibility of undue pressure being developed between cutter and crystal, owing to injudiciously rapid rotation of the traversing screw, is thus avoided, the spring giving way before the pressure becomes dangerously great and the piston being pulled out of the cylinder. Two inches of traverse are permitted by the length of the slot *z* of the fork and of the traversing bed of the slider, an amount ample to permit of the cutting through of a crystal an inch in diameter.

This form of cutting apparatus is found to work admirably; there is no tendency to jamming when once the operator has become familiar with the rate at which the milled head of the traversing apparatus can most advantageously be rotated, and the cutting is more perfectly under control than when the pressure of the cutter against the crystal is brought about by a spring or a weight hanging over a pulley.

The adjustment of the crystal, so that the direction of the desired surface is exactly parallel to the cutting disc and grinding lap, is carried out as described in the former communication. When about to commence cutting, the crystal is first lowered by means of the milled head at the top of the instrument until it is at a convenient level for cutting, in which position it is fixed by first clamping the circle to the fixed cone by means of the milled headed screw forming part of the arrangement for fine adjustment, and then preventing any movement of the gun-metal axis by tightening the collar immediately above the circle by means of a key provided. Cutting is then proceeded with, commencing rotation of the cutter and its traversing slowly at first, and gradually increasing the speed according to the "feel" of the cutting. If by inadvertence a jam should ever occur, a slight reversal of the motion of the traversing screw will instantly release the cutter. The speed should be materially reduced towards the finish, in order that the end of the crystal may be cut off cleanly right up to the furthest edge. A hard crystal such as a topaz, ½ in. thick, may be easily cut through in 10 to 15 minutes, and the cut surface is so smooth that very little grinding is required, which may at once be proceeded with upon the gun-metal or hard white-metal lap, with the finest washed flour emery made into a moderately thick paste with water or brick-oil. The latter lubricant offers the advantage of greater immunity from splashing, although not perhaps so convenient in other respects.

A thick glass disc, about 1 in. in diameter, is provided for use as a "bruiser," to work down the emery paste to an even consistency upon the lap before grinding. The pressure between crystal and lap is controlled by manipulation of the counterpoised levers, as described in the former memoir, and the path of the crystal upon the lap changed from time to time by use of the centering movements, in order to prevent undue local wearing of the lap.

A further useful accessory is a small stand consisting of a weighted base carrying an upright tubular stem, in which a lengthening rod is capable of sliding, which latter carries an adjustable support, formed by a couple of short links united by ball-and-socket joints, for an oblique caoutchouc wedge. The latter, when directed down upon the lap, not quite in contact with the metallic surface of the latter, serves to keep the emery paste moving into the path of the crystal during grinding. When soft artificial crystals are being ground upon the glass laps, the wedge may be replaced by a camel-hair brush, to keep the path well supplied with the lubricating oil.

The surfaces prepared by the fine grinding are usually sufficiently transparent for the purposes of optical investigations, plates perpendicular to the median lines exhibiting clearly defined interference figures in convergent polarised light. If a higher polish is desired, the polishing laps supplied will enable this to be attained.

After the completion of the operations upon the first surface, the cutting of the second is proceeded with while the crystal is still adjusted, the latter being fixed at the suitable height for the purpose. The plate thus cut off is cemented by the finished surface to one of the small glass discs provided with the special holder, and the finishing of the second surface carried out with the disc supported in the holder, whose true plane has previously been adjusted parallel to the lap, as described in the former communication.

When a prism is to be prepared the crystal is first adjusted so that the plane of optical elasticity to which the two desired prism faces are to be symmetrical is parallel to the grinding lap, and likewise so that the direction of the desired edge, parallel to an axis of optical elasticity, is at right angles to the upper tangent screw of the adjusting apparatus. The latter is then rotated 30°, the crystal arranged at the convenient height for cutting, and the end cut off. The cut surface is then finished upon the laps. The tangent screw is next rotated back to the adjusted position, and for 30° upon the other side of that, when, after fixing at the proper height, the second surface is cut. The 60° prism thus cut off is again set in wax on the holder with the second surface exposed, and the latter is then brought parallel to the lap by goniometrical adjustment, and ground and polished, if desired, in like manner to the first. The two surfaces will invariably afford brilliant single reflected images of the spectrometer slit, and transmit equally well-defined refracted images when arranged for minimum deviation.

Instead of actuating the driving gear of the cutting or grinding apparatus by hand, which, however, is very light labour owing to the freedom from friction, a small electric, gas, or water motor may be employed. For this purpose the driving wheel α of the grinding apparatus is supplied with two annular niches, the upper of which is intended for the reception of the band from the motor; and two similar driving pulleys, one, β, for hand rotation, and the other, γ, for use with the motor, are fitted at the two ends of the driving axle of the cutting apparatus, that for hand driving being above the arm at the level of the other pulleys of the cutting apparatus, and that for the reception of the band from the motor being arranged below the arm, at the level of the upper niche of the grinding pulley. A small electric motor, driven by the current from three pint bichromate cells, affords ample power for the purpose, and the speed of revolution is best controlled by a friction brake upon the motor.

The instrument has been constructed by Messrs. Troughton and Simms, who are prepared to furnish copies of either this or the smaller instrument. The author desires to express his thanks to Mr. Skinner, of the Charlton Works, for invaluable assistance in devising it, and to Mr. F. Chapman, of the geological laboratory of the Royal College of Science, for the benefit of his large experience in the cutting and grinding of minerals. The first model of the instrument is exhibited in the Science Division of the South Kensington Museum.

V. "Note on the Disease of Cabbages and allied Plants known as 'Finger and Toe,' &c." By GEORGE MASSEE, a Principal Assistant, Royal Gardens, Kew. Communicated by W. T. THISELTON DYER, Esq., F.R.S., Director. Received January 25, 1895.

The disease known in different parts of Britain as "finger and toe," "clubbing," or "anbury," attacks turnips, rape, cabbages of all varieties, radishes, and, in fact, most cultivated plants belonging to the order *Cruciferæ*. Several common weeds are also attacked, namely, charlock (*Brassica Sinapistrum*, Boiss.), garlic-mustard (*Sisymbrium Alliaria*, Scop.), treacle-mustard (*Erysimum Cheiranthoides*, Linn.), and shepherd's purse (*Capsella bursa-pastoris*, D.C.). The last-named is reported from the United States by Halsted,* and has not been observed to be diseased in Britain, although one of our commonest weeds. The disease is characterised by the formation of numerous nodules on the root, which becomes much distorted and soon decays, forming a slimy, fœtid mass.

Berkeley† appears to have been the first to investigate the disease from a scientific standpoint, and although he did not succeed in determining the true cause, distinctly states that microscopic examination revealed the presence of a factor previously unknown in connection with plant diseases. Furthermore, Berkeley pointed out that wood ashes were a cure for the disease, and supposed this to be due to the presence of potash salts in the ash.

Owing to the serious amount of damage caused by "finger and toe" to the cabbage crop in Russia, the Government of that country offered a reward for the discovery of the cause of the disease. Woronin‡ undertook the investigation, and after years of patient study published an elaborate account, proving clearly that the disease was caused by a minute organism related to the fungi, to which he gave the name *Plasmodiophora brassicæ*.

In 1859, Voelcker§ pointed out that the disease was influenced by the amount of lime present in the soil. Where little or no lime existed, as in light and sandy soils, the disease abounded, whereas in soil containing lime the disease was absent. This opinion is corroborated by the same author at a later date.*

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* New Jersey Agric. Coll. Expt. Station; Bull. 98 (1893).

† 'Gard. Chron.,' p. 500, 1856.

‡ 'Pringsheim's Jahrb.,' vol. 11, p. 548, tabs. xxix—xxxiv (1878).

§ 'Roy. Agric. Soc. Journ.,' vol. 20, p. 101 (1859).

The following is an account of experiments conducted during four successive years at Kew.

1. Healthy seedling cabbages planted in soil that had two years previously produced a crop of diseased cabbages, became diseased. Check plants from the same batch of seed, grown in sterilised soil, remained free from disease. Somerville† has also demonstrated that turnips become diseased when the seed is sown in soil brought from an infected area.

A. *Experiments conducted in a Sterilised Solution of Stable Manure.*

2. The contents of two flasks were infected by adding crushed tubercles of a diseased cabbage root. 2 per cent. of a saturated solution of potassium hydrate was added to the contents of one flask, and 2 per cent. of commercial sulphuric acid to the other. A young cabbage plant free from disease was then placed in each flask. At the end of two months the plant in the flask containing potassium hydrate was growing vigorously and perfectly free from disease, whereas the plant growing in the solution containing sulphuric acid was badly diseased, much more so than check plants growing in infected soil free from acid, for the same period of time. Similar experiments made during successive years always yielded the same result.

3. Two young cabbage plants showing decided symptoms of disease were placed in flasks containing the same proportions of potassium hydrate and sulphuric acid respectively as in Experiment 1. At the end of two months the plant growing in the solution containing potassium hydrate appeared to be perfectly healthy, the indications of nodules present on the root at the commencement of the experiment having become effaced by subsequent growth. The plant growing in the solution containing sulphuric acid was badly diseased.

Similar results were obtained in Experiments 1 and 2, when potassium hydrate was replaced by ammonium hydrate, and sulphuric acid by hydrochloric acid.

4. Two diseased seedling cabbages were placed in separate flasks of the sterilised solution. The liquid in one flask was saturated once a week with carbonic dioxide, the contents of the second flask not being interfered with in any way. At the end of two months both plants had the disease developed to the same extent, proving that carbonic acid is neutral as regards the development of *Plasmodiophoræ*.

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* *Op. cit.*, Ser. III, vol. 5, p. 321 (1894).

† *Op. cit.*, Ser. III, vol. 5, p. 808 (1894).

B. *Experiments conducted with Sterilised Soil.*

5. Two pots of soil, sterilised by steam, were infected with the crushed roots of diseased cabbages. The soil in one pot was mixed with quicklime, that in the other with bone manure, having an acid reaction. A healthy cabbage seedling was planted in each pot, and at the end of two months the plant in the pot containing lime was perfectly healthy, whereas the plant in the soil containing acid bone manure was badly diseased.

6. Two pots containing soil mixed with quicklime and acid bone manure respectively had a diseased cabbage seedling placed in each. At the end of two months the disease was more developed in both seedlings than at the time of planting; proving that the presence of lime will not arrest the disease when the plants are once attacked.

Summary.

The foregoing observations and experiments demonstrate the following points:—

1. That in addition to cultivated plants, several common weeds belonging to the order *Cruciferæ* are attacked by the *Plasmodiophora.* Hence the necessity for preventing the growth of such weeds in fields and hedge-banks.

2. That the germs of disease are present in soil that has produced a diseased crop, and retain their vitality for at least two years.

3. That the development of *Plasmodiophora* is favoured by the presence of acids, and checked by the presence of alkalies, agreeing in this respect with the fungi rather than with bacteria.

4. For the purpose of sterilising infected soil, experiments prove that either a dressing of lime or a manure containing potash salts is effective, the last being most valuable, as it not only destroys the germs in the soil but also arrests the disease in seedling plants, and at the same time supplies one of the ingredients necessary for the healthy growth of turnips.

Presents, February 14, 1895.

Transactions.

Baltimore:—Johns Hopkins University. Circulars. Vol. XIV. No. 116. 4to. *Baltimore* 1895. The University.

Berlin:—Gesellschaft für Erdkunde. Verhandlungen. Band XXI. No. 1. 8vo. *Berlin* 1895. The Society.

Boston:—Society of Natural History. Memoirs. Vol. III. No. 14. 4to. *Boston* 1894; Proceedings. Vol. XXVI. Parts 2, 3. 8vo. *Boston* 1894; Occasional Papers. No. 4. 8vo. *Boston* 1894. The Society.

Transactions (*continued*).

Buffalo :—Society of Natural Sciences. Bulletin. Vol. V. No. 4. 8vo. *Buffalo* 1894. The Society.

Cambridge, Mass. :—Museum of Comparative Zoology. Memoirs. Vol. XVII. No. 3. 4to. *Cambridge, Mass.* 1894. The Museum.

Chicago :—Field Columbian Museum. Pub. 1. Vol. I. No. 1. 8vo. *Chicago, U.S.A.* 1894. The Director.

Iowa :—Academy of Sciences. Proceedings for 1893. Vol. I. Part IV. 8vo. *Des Moines* 1894. The Academy.

Kew :—Royal Gardens. Bulletin of Miscellaneous Information. No. 96. 1894. 8vo. *London.* The Director.

Klausenburg :—Erdélyi Múzeum Egylet. Orvos-Természettudományi Értesitő. Vol. XIX. 1894. Orvosi Szak. Füzet I—III; Természettudományi Szak. Füzet III. 8vo. *Kolozsvárt* 1894. The Society.

London :—Entomological Society. Transactions. 1894. Part V. 8vo. *London.* The Society.

Milan :—R. Istituto Lombardo di Scienze e Lettere. Memorie. Vol. XVI. Fasc. III. 4to. *Milano* 1891. The Institute.

Società Italiana di Scienze Naturali. Memorie. Tomo V. 4to. *Milano* 1895. The Society.

Moscow :—Société Impériale des Naturalistes. Bulletin. Année 1894. No. 3. 8vo. *Moscou.* The Society.

New York :—Academy of Sciences. Transactions. Vol. XIII. 1893–94. 8vo. *New York* [1895]. The Academy.

Medico-Legal Society. Psychological Section. Bulletin. Vol. 2. Nos. I—III. 8vo. *New York* 1894. The Society.

Philadelphia :—Academy of Natural Sciences. Journal. Second Series. Vol. X. Part 2. Folio. *Philadelphia* 1894; Proceedings. 1894. Part II. 8vo. *Philadelphia.* The Academy.

St. Petersburg :—Académie Impériale des Sciences. Bulletin. V^e Série. Tome II. No. 1. 4to. *St. Pétersbourg* 1895. The Academy.

Salem :—American Association. Proceedings. 42nd Meeting held at Madison, Wisconsin. August, 1893. 8vo. *Salem* 1894. The Association.

Trencsén :—Naturwissenschaftliche Verein. Jahrgang XIV—XV. 1892–93. 8vo. *Trencsén* 1893. The Society.

Washington :—U.S. Fish Commission. Bulletin. Vol. XII. 1892. Large 8vo. *Washington* 1894. The Commission.

U.S. National Museum. Proceedings. Vol. XVI. 1893. 8vo. *Washington* 1894; Report. 1892. 8vo. *Washington* 1893. The Museum.

Observations and Reports.

Amherst (Mass.):—State Agricultural Experiment Station. Reports and Bulletins. Vols. I—XI. 8vo. *Boston* 1884–94. The Station.

Brussels:—Observatoire Royal de Belgique. Annuaire. 1895. 62^e Année. 12mo. *Bruxelles.* The Observatory.

Cambridge, Mass.:—Harvard College. Astronomical Observatory. Annals. Vol. XXXV. 4to. *Waterville, Me.* 1894; Vol. XL. Part III. Vol. XLI. No. 1. 4to. *Cambridge* 1894. The Observatory.

Mount Hamilton:—Lick Observatory. Publications. Vol. III. 1894. 4to. *Sacramento.* The Observatory.

Rome:—Ministero di Agricoltura, &c. Biblioteche dello Stato, delle Provincie, dei Comuni, &c. Vol. I—II. 8vo. *Roma* 1893–94. The Ministry.

R. Accademia dei Lincei. Annuario. 1895. 12mo. *Roma.* The Academy.

St. Petersburg:—Kaiserliche Akademie der Wissenschaften. Supplementband zum Repertorium für Meterologie. No. 6. 4to. *St. Petersburg* 1894. The Academy.

Physikalisches Central-Observatorium. Annalen. Jahrgang 1893. Theil 1. 4to. *St. Petersburg* 1894. The Observatory.

Wisconsin:—Washburn Observatory. Publications. Vol. VII. Part 2. 8vo. *Madison, Wis.* 1894. The Observatory.

Journals.

Boletin de Minas Industria y Construcciones. Año X. Nos. 6—9. 4to. *Lima* 1894. Escuela Especial de Ingenieros, Lima.

Medico-Legal Journal. Vol. XII. No. 2. 8vo. *New York* 1894. The Editor.

Studies from the Laboratory of Physiological Chemistry, Sheffield Scientific School of Yale University. Vols. I—III. 8vo. *New Haven* 1885-89. The Editor.

Zeitschrift für Naturwissenschaften. Band 67. Heft 5. 8vo. *Leipzig* 1894. Naturwissenschaftlicher Verein, Halle.

Beddoe (Dr. John) Sur l'Histoire de l'Index Céphalique dans les Iles Britanniques. 8vo. *Paris* [1895]. The Author.

Bell (Clark) Medico-Legal Studies. Vol. III. 8vo. *New York* 1893. The Author.

Tommasi (Prof. Annibale) Rivista della Fauna Raibiiana del Friuli. 8vo. *Udine* 1890; La Fauna del Calcare Conchigliare (Muschelkalk) di Lombardia. 8vo. *Pavia* 1894. The Author.

February 21, 1895.

The LORD KELVIN, D.C.L., LL.D., President, followed by Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

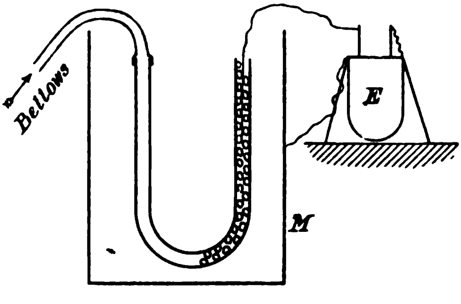
The following Papers were read:—

I. "Electrification of Air and other Gases by bubbling through Water and other Liquids." By LORD KELVIN, P.R.S., MAGNUS MACLEAN, M.A., F.R.S.E., and ALEXANDER GALT, B.Sc., F.R.S.E. Received February 7, and February 15, 1895.

§ 1. At the meeting of the British Association in Oxford in August, 1894, a communication was given to section A, entitled "Preliminary Experiments to find if Subtraction of Water from Air Electrifies it." These experiments were performed during July of 1894, and were a continuation of experiments which were commenced in the Physical Laboratory of the University of Glasgow in December of 1868 with the same object, but which were then, for various reasons, discontinued before any decisive result had been obtained.

§ 2. A glass U-tube with vertical branches (fig. 1), each 18 inches long and about 1-inch bore, with the upper 8 inches of one of the branches carefully coated outside and inside with clean shellac varnish, was held fixed by an uninsulated support attached to the upper end of this branch. The other branch was filled with little fragments of pumice soaked in strong sulphuric acid or in water, and a fine

FIG. 1.

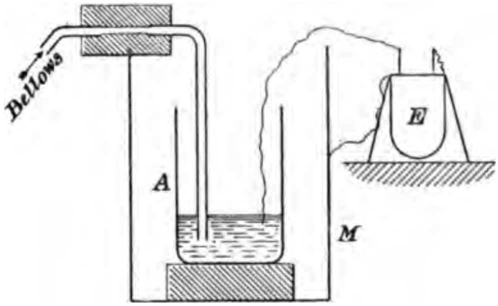


platinum wire, with one end touching the pumice, connected it to the insulated electrode of a quadrant electrometer. A metal can, M, large enough to surround both branches of the U-tube without touching either, was placed so as to guard the tube from electric influences of surrounding bodies, the most disturbing of which is liable to be the woollen cloth sleeves of the experimenters or observers moving in the neighbourhood. This metal can was kept in metallic connection with the outside metal case of the quadrant electrometer. The length of the exposed platinum wire between the U-tube and the electrometer was so short that it did not need a metal screen to guard it against irregular influences. An india-rubber tube from an ordinary blow-pipe bellows was connected to the uninsulated end of the U-tube. Air was blown through it steadily for nearly an hour. With the pumice soaked in strong sulphuric acid in the other branch, the electrometer reading rose in the course of three-quarters of an hour to about 9 volts positive. *When the pumice was moistened with water, instead of sulphuric acid, no such effect was observed.* The result of the first experiment proves decisively that the passage of the air through the U-tube gave positive electricity to the sulphuric acid, and therefore sent away the dried air with negative electricity. A corresponding experiment with fragments of pure chloride of calcium instead of pumice in sulphuric acid, gave a similar result. In repetition of the experiments, however, it was noticed that the strong positive electrification of the U-tube seemed to commence somewhat suddenly when a gurgling sound—due to the bubbling of air through free liquid, whether sulphuric acid or chloride of calcium solution in the bend of the U-tube—began to be heard. It has since been ascertained that it was because no liquid accumulated in the bottom of the U-tube that no electric effect was found when the pumice was moistened with pure water.

§ 3. Arrangements were made to prevent any bubbling of the air through liquid, by using a straight tube instead of a U-tube. In a large number of experiments with pumice, moistened with pure sulphuric acid, in the straight tube, and air blown through for about half an hour, no definite electrification was obtained. In this straight tube, as formerly with the U-tube, pumice moistened with pure water gave no electrification. Chloride of calcium in lumps, not specially dried, gave no effect in the straight tube; but if previously heated to 180° or 200° and put into the straight tube when still hot, it gave an enormous positive electrification immediately on the commencement of blowing. Strong positive electrification was obtained a second time, by discharging the electrometer to zero, re-insulating, and re-commencing the blowing. But after discharging a second time, re-insulating, and re-commencing the blowing, no further electrification was found.

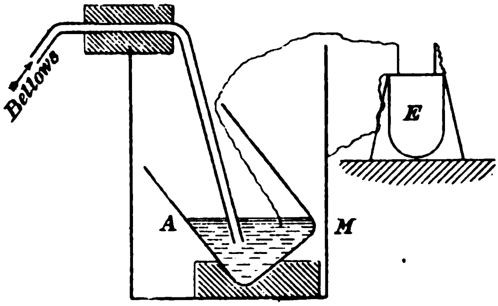
§ 4. In continuation of these experiments, on the 25th of September the arrangement represented in fig. 2 was set up. An outer metallic guard-vessel, M, was kept connected by a wire to the case and to one pair of quadrants of a quadrant electrometer, E. Water in an inner glass or metal jar, A, was connected by a platinum wire to the other pair of quadrants of the electrometer. To have this inner jar well insulated, it was supported on a block of paraffin; and the upper end of a glass tube dipping into the water was fitted into one end of a tube of paraffin, to the other end of which was fitted a tube for ingress of air, from bellows, as shown in the figure. The insulation of this arrangement was found to be good. When air was blown through the water it was found that the jar containing the water became positively electrified.

FIG. 2.



§ 5. To prevent splashing of water out of the jar, a paper cover was put on its mouth, or the jar was tilted, as shown in fig. 3, so that the bubbles broke against the inside of the jar. In three experiments thus made, the same electrification was still found, amounting to about 6 volts positive in a quarter of an hour.

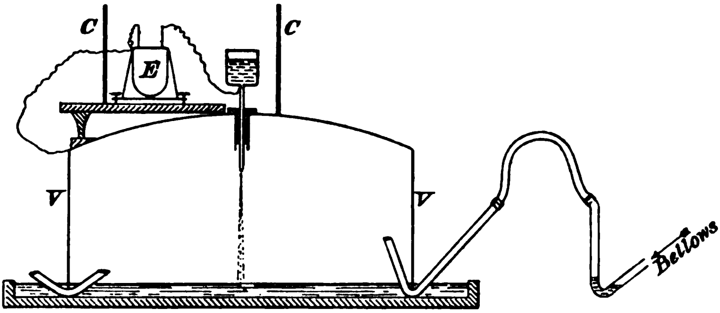
FIG. 3.



§ 6. As the jar was in every experiment positively electrified, the air, if unelectrified* when entering it, must have been negatively electrified when leaving it.

§ 7. To test if the air was negatively electrified after bubbling, on the 11th of October the apparatus† shown in fig. 4 was set up. The apparatus consists of a large sheet iron vat, VV, 123 cm. in diameter and 70 cm. in height, inverted on a large wooden tray lined with lead, and supported by three blocks of wood. By filling the tray with water, the air is confined in the vat.

FIG. 4.



CC is a metal screen kept metallically connected with the case of the electrometer, and with the vat. It surrounds both the electrometer and the water-dropper, to prevent any external varying electrifications from vitiating the proper results of our experiments.

This screening of the electrometer is absolutely necessary when it is used with high sensibility (70 scale divisions per volt in our experiments) in a laboratory or other place where various other electric experiments may simultaneously be going on. Four years ago the electrometer, the vat, and the water-dropper, were set up on the class-room table *without a metal screen.* When the deflection indicated about 4 volts negative (see § 8), the negative lead of Lord Kelvin's house electric-light circuit, which passes through the class-room, was joined to earth. This changed the deflection of the electrometer suddenly by 1 volt in the positive direction. When the positive lead was "earthed," the deflection was changed suddenly by 6 volts in the negative direction. Putting on sixteen 8 c.p. electric lamps, eight on each side of the class-room, changed the deflection by two-thirds of a volt in the negative direction.

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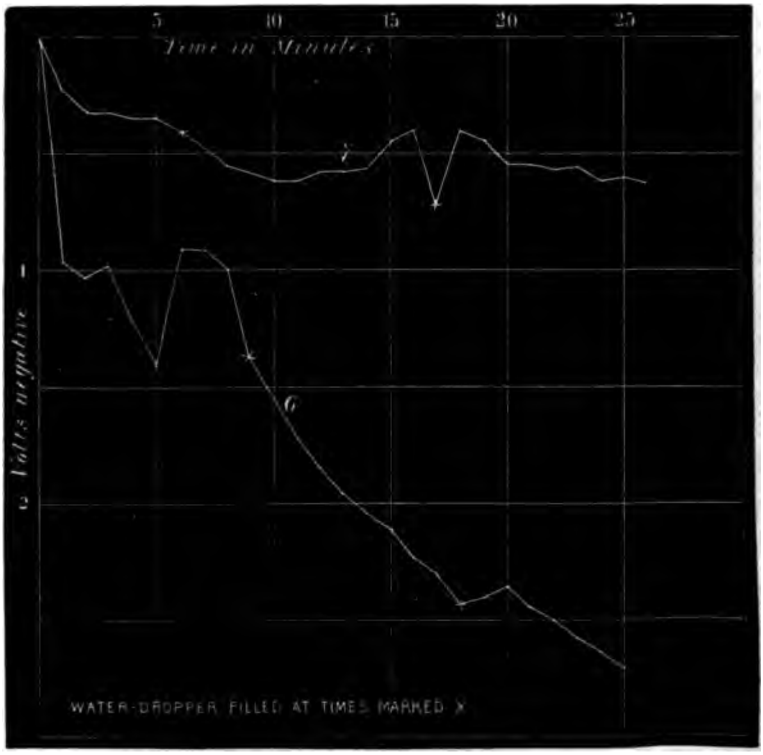
* Air was similarly blown from bellows into the vat (see § 7) without any bubbling, and no electrification was observed.

† The vat, the water-dropper, and the electrometer are the same as in the apparatus described in the 'Proceedings of the Royal Society,' vol. 56, year 1894, "Electrification of Air," by Lord Kelvin and Magnus Maclean.

§ 8. In experimenting with the same apparatus* in 1890 it was found that the water jet gave negative electricity to the ordinary air of the laboratory enclosed in the vat. The present experiments fully confirm this result, showing a gradual negative electrification of the enclosed mass of air rising to about 5 volts in an hour, once every day for the first few days. For twenty-eight days after the vat was set up in October, 1894, fifteen observations of an hour each were taken, to find the effect of the water-dropper, with no other disturbing influence on the unchanged volume of air inside the vat. These experiments verify the conclusion ('Phil. Mag.,' August, 1890) that the more the air inside the vat became free of dust, the less became the rate at which the air was negatively electrified by the water-dropper.

§ 9. On the 15th October last the vat was lifted from the tray to remove some obstruction in the nozzle of the water-dropper, which was not then flowing freely. Curve (6) was obtained that afternoon.

CURVES 6 AND 7.



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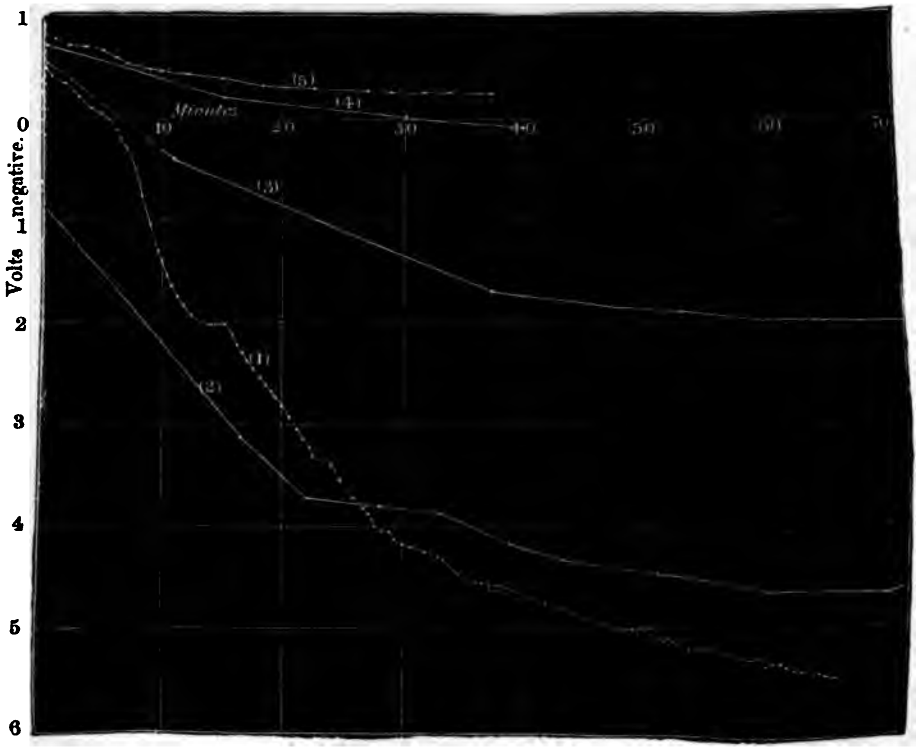
* 'Phil. Mag.,' August, 1890, "Electrification of Air by a Water Jet," by Maclean and Goto.

The air in the vat was the ordinary air of the laboratory, and the curve shows the effect of the water-dropper alone in electrifying the air negatively. For the next two days the water-dropper was kept running continuously for about eight hours each day, to wash the dust out of the air, and on the 18th of October curve (7) was obtained. It shows a much less rate of negative electrification than curve (6). In the experiments of summer 1890 an aspirator was used to draw the air from the vat, and a tube full of cotton-wool was used to filter the air drawn into the vat.

Curves (1) to (5) are reproduced from the 'Philosophical Magazine,' and they show that the more the air becomes free from dust the less is the rate at which the water-dropper electrifies. Thus curve (1) was obtained from the ordinary air of the laboratory, in the vat, and curve (2) after the aspirator was working for some time. In this curve the water-dropper itself was running for some time before the first observation was taken. The other curves were obtained after further continuous working of the aspirator.

Curves 1 to 5.

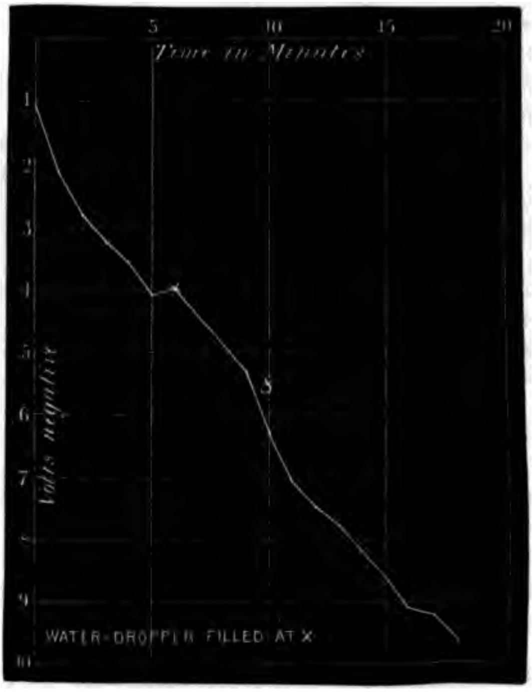


After curve (4) was obtained the aspirator was worked continuously for twenty-five hours, and then curve (5) was obtained.

§ 10. At the end of twenty-three days of October and November 1894 (§§ 8, 9 above), when the air inside the vat must have been fairly free from dust, and when the water-dropper of itself was giving little negative electrification, we bubbled air into it by a forked tube, one end of which was connected to a bellows, and three other open ends were below the water inside the vat. In five experiments thus made—two on November 7, two on November 8, and one on December 17—an average negative electrification of 5 volts in twelve minutes was obtained.

§ 11. We now arranged a U-tube with pure water in it (fig. 4) outside the vat. Air from the bellows bubbled through the water in this U, and was carried thence by a block-tin pipe into the vat without any further bubbling. Observations by the quadrant electrometer, while the water-dropper was running and the bellows worked, gave us measurements of the varying state of the electrification of the air in the vat. The average of fifteen experiments gave a negative electrification of the air in the vat of 8½ volts in 25

CURVE 8.



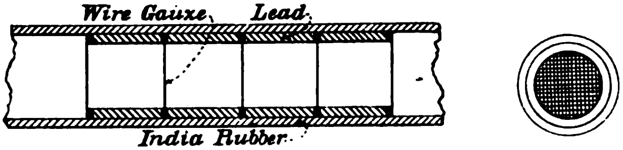
minutes. The rate at which the air was blown in in these experiments was such as to displace the entire volume of air in the vat in half an hour.

§ 12. Curve (8) shows the rate of electrification of air, in one of the fifteen experiments, when thus bubbled through the water in the U-tube and then admitted into the vat.

§ 13. Two U-tubes, in series, with water in each, did not seem to give a perceptibly cumulative effect.

§ 14. The effect of one or more wire gauze strainers between the U-tube and the vat, or between the U-tube and the bellows, was next tested. The gauzes were placed between short lengths of lead tube, which were held together by a rubber tube slipped over them. The arrangement is shown by longitudinal and cross sections in fig. 5.

FIG. 5.



Twelve wire gauzes, with or without cotton-wool between them, placed between the bellows and the U-tube, did not prevent the subsequent electrification by bubbling of the air thus filtered. But when placed between the U-tube and the vat they almost entirely diselectrified the air, even without the cotton-wool, and still more decidedly when cotton-wool was loosely packed between the wire gauzes. A single wire-gauze strainer produced but little of dis-electrifying effect.

§ 15. The interpretation of these experiments is complicated, and the time required for each is lengthened, on account of the large mass of air in the vat to start with, whether uncharged or retaining electricity from previous experiments, and also on account of the effect of the water-dropper itself. Hence, in our later experiments, we fell back on the arrangement shown in fig. 2, by which we test the electrification of the liquid, and not directly that of the gas blown through it.

§ 16. In our first experiments with this apparatus the amount of the electrification did not seem much affected when a paper cover was put on the jar, or when we tilted the jar as shown in fig. 3. We now made a large number of tests with different covers and screens (chiefly of sheet copper or sheet zinc, or brass wire gauze) at different heights above the liquids, and we concluded that, if the screens are not within a centimetre and a half of the liquid surface, they do not directly affect the magnitude of the electrification obtained. In nearly all of the subsequent experiments a horizontal circular screen of thin sheet copper, leaving an air space of about 3 mm. all round between its edge and the inner surface of the jar, about 3 cm. above the liquid surface, was used to prevent spherules of the liquid from being tossed out of the jar by the bubbling.

§ 17. In the following short summary of our results the duration of each experiment was 10 minutes. The effect of blowing air through water and other liquids is summarised in §§ 18 to 27, and of blowing other gases than air through water in §§ 28 to 31.

§ 18. The jar contained 200 c.c. of the Glasgow town-supply water (from Loch Katrine). A mean of seventeen experiments showed an electrification of the jar to 4 volts positive when air was blown through it for 10 minutes.

§ 19. A solution of zinc sulphate of different strengths was now used instead of the pure water. Three experiments, with 150 c.c. of water containing one drop of a saturated solution of the zinc sulphate, gave half the positive electrification that would, under similar circumstances, have been obtained from water only. With five drops no definite electrification was obtained. With greater proportions of the zinc sulphate solution up to saturation (twenty-four experiments altogether) the electrification was on the average slightly negative.

§ 20. Twelve experiments were then made to test the effect of adding a solution of ammonia to the water. One drop reduced the electrification to one-half; two drops brought it down to one-quarter. With larger proportions of ammonia than this, up to a saturated solution, we found a very slight positive electrification, never amounting to more than a small fraction of a volt, and therefore negligible in the circumstances.

§ 21. Seven experiments with sulphuric acid of different strengths all showed small *positive* electrification, the amount gradually decreasing from $\frac{1}{4}$ volt, in 10 minutes, with 0·5 per cent. acid in water to $\frac{1}{16}$ volt, in the same time, with acid of full strength.

Seven experiments with hydrochloric acid solution of different strengths all showed a small *negative* electrification, the amount gradually increasing from $\frac{1}{2}$ volt, in 10 minutes, with $\frac{1}{3}$ per cent. acid solution in water to $1\frac{1}{4}$ volts, in the same time, with acid solution of full strength.

Nine experiments with calcium chloride solution were made. A saturated solution and a solution diluted to 75 per cent. of full strength gave no result; but solutions of gradually diminished strength, from 50 per cent. down to $\frac{1}{10}$ per cent., showed a negative electrification from fully $\frac{1}{2}$ volt, in 10 minutes, down to $\frac{1}{13}$ volt.

Additions of very small quantities of washing soda to water greatly reduce the positive electrification obtained.

Loch Katrine water, supersaturated with carbonic acid, and placed in the insulated jar, showed, when air was bubbled through it for ten minutes, a negative electrification of $\frac{1}{4}$ volt.

§ 22. Ten drops of paraffin oil added to water reduced the electrification to about half of that obtained from water only. Thirty drops reduced it to about a tenth, which as it amounted to only 0·4 volt during the time of the experiment is negligible.

§ 23. Ten drops of benzene reduced the electrification to half, and thirty drops to about a third of that taken by pure water.

§ 24. A saturated solution of granulated phenol (carbolic acid) was made, and small portions of it added to the water in the jar. Several experiments showed no diminution in the electrification as long as the quantity of the phenol solution present in the water was under 10 per cent. With 25 per cent. the electrification was reduced to a third. With strengths greater than this up to saturation the electrification was reduced to one-sixth.

§ 25. A saturated solution of common salt was prepared. Blowing air through 200 c.c. of water containing the quantities of the salt solution mentioned, gave us in 10 minutes the following electrifications:—

(*a*.)	0·004	per cent. of saturated solution of salt in water..	2·4	volts positive.
(*b*.)	0·02	,, ,, ..	1·2	,,
(*c*.)	0·1	,, ,, ..	0·6	,,
(*d*.)	0·5	,, ,, ..	0·4	,,
(*e*.)	2·0	,, ,, ..	0·15	,,
(*f*.)	4·0	,, ,, ..	0·0	,,

§ 26. Several experiments showed that with 200 c.c. of water containing not more than ten drops of absolute alcohol, practically the same amount of positive electrification (4 volts in ten minutes) is obtained as if pure water were used. With fifty drops less than 2 volts were got, and with 100 drops less than 1 volt. 25 and 50 per cent. alcohol in the water gave very small and hence negligible positive electrification.

§ 27. One drop of saturated solution of copper sulphate in 200 c.c. of water showed 1 volt positive in 10 minutes. With $\frac{1}{2}$ per cent. of it in the water, the electrification was reduced to a fraction of a volt positive. With greater proportions of copper sulphate present, up to saturation, slightly negative electrifications were obtained, but never amounting to more than about one-tenth of a volt, and hence negligible.

§ 28. On blowing carbonic acid gas, from a cylinder obtained from the Scotch and Irish Oxygen Company, through pure water in the glass jar, the water became electrified to $8\frac{3}{4}$ volts positive in ten minutes. Blowing the breath through water gave an electrification of 3 volts positive in the same time: this diminished result is doubtless due chiefly to the diminished rate of bubbling.

§ 29. The blowing of oxygen from a cylinder, obtained from the Oxygen Company, through water, gave as a mean of four experiments, a positive electrification to the water of half a volt in 10 minutes. When continued for 55 minutes, it gave the very decided result of 5 volts positive.

§ 30. Hydrogen prepared from zinc and dilute sulphuric acid was passed into a large metal gas-holder; and was passed on from this to bubble through the water in the insulated jar. In two experiments this was done immediately after the preparation of the hydrogen; in another it was done after the hydrogen had remained 18 hours in the gas-holder. In each of the three experiments the water was electrified to 2 volts positive in 10 minutes.

When the hydrogen was allowed to pass direct through a tube from the Wolffe's bottle where it was generated, to bubble in the insulated jar, the magnitude of the effect obtained was very much larger. In one case a mixture of muriatic acid and sulphuric acid and water was used, and the reading went off the scale positive in 30 seconds (more than 10 volts). In other two experiments, with dilute sulphuric acid and zinc in the Wolffe's bottle, the electrifications obtained were 6 volts positive in 7 minutes, and 7·3 volts positive in 13 minutes, in the last of which the hydrogen was allowed to bubble through caustic potash contained in a small bottle between the Wolffe's bottle and the insulated jar.

The hydrogen was next generated in the insulated jar itself, the tube for ingress of air used in the ordinary experiments being taken away. 200 c.c. of pure water, along with some granulated zinc, was put into the jar. Then some pure sulphuric acid was added, and electrometer readings were taken. In two experiments with no screen in the jar (§ 16) the reading went off the scale *negative* (1) in 2 minutes and (2) in 4 minutes (more than 9 volts in each case). In another experiment, in other respects the same, but with a copper screen 7 cm. above the surface of the liquid, the electrification showed 2 volts *negative* in 2 minutes, then came back to zero in 5 minutes, and in the next 6 minutes went 4 volts *positive*. The jar and pair of quadrants connected with it were then metallically connected with the outer case of the electrometer for a few seconds, and reinsulated; in 5 minutes the reading went up to 2 volts *positive*. A little more sulphuric acid was added to the jar, which was disinsulated for a short time and reinsulated; the reading went up to 7 volts *positive* in 4 minutes. The jar was again disinsulated for a few seconds and reinsulated; the reading went up in 4½ minutes to 6½ volts *positive*.

§ 31. Coal-gas, bubbled through water in the insulated jar, gave 1·4 volts positive in 10 minutes.

§ 32. In the ordinary experiment of bubbling air through a small quantity of water in the bottom of the jar it was noticed that the electrification did not commence to be perceptible generally till about the end of the first minute; and that it went on augmenting perceptibly for a minute or more after the bubbling was stopped. The following experiment was therefore tried several times. One of us stood leaning over the jar, with the head about 10 ins. above it, and the mouth so partly closed that breathing was effected sideways; another blew the bellows; and another took the readings of the electrometer. After bubbling had been going on for some minutes, and the readings were rising gradually (4 volts per 10 minutes, as in § 18), blowing was stopped. As soon as the bubbling ceased, the first-mentioned observer, without moving his head or his body (see § 7, regarding the necessity to have the electrometer screened from outside influences) blew into the jar to displace the negatively electrified air in it. In every case the electrometer reading showed instantly a small rise in the positive direction.

In the carrying out of these experiments we have received much valuable help from Walter Stewart, M.A., and Patrick Hamilton, B.Sc.

§ 33. The very interesting experiments described by Lenard, in his paper on the Electricity of Waterfalls,* and by Professor J. J. Thomson, on the Electricity of Drops,† show phenomena depending, no doubt, on the properties of matter to which we must look for explanation of the electrical effects of bubbling described in our present communication, and of the electrification of air by drops of water falling through it, to which we have referred as having been found in previous experiments which were commenced in 1890 for the investigation of the passage of electrified air through tubes.‡

II. "Note on the Spectrum of Argon." By H. F. NEWALL. Communicated by LORD RAYLEIGH. Received February 14, 1895.

In the course of a spectroscopic investigation in which I have been for some time past engaged, a line spectrum, which so far as I was able to make out was unknown, has frequently presented itself upon my photographs. It appeared in May and June, 1894, under conditions which led me to call it, for the sake of convenience, "the low-pressure spectrum." After their announcement at the Oxford meeting of the British Association, it seemed for many reasons natural to borrow the first letter of Lord Rayleigh's and Professor Ramsay's names to give to the unknown lines, and in the measurements of the photographs which showed the lines well, there appears an "R" against seventeen lines out of sixty-one measured, the remaining lines being known to belong to Hg, H, N, and nitrocarbons. It transpires now, as I learnt from reading the abstract of the paper in which Lord Rayleigh and Professor Ramsay describe their consummate researches on argon, that the symbol "A" should have been used instead of "R" to designate the lines on my photographs. For the lines are Argon lines.

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* 'Wiedemann's Annalen,' 1892, vol. 46, pp. 584—636.

† 'Phil. Mag.,' April, 1894, vol. 37, pp. 341—358.

‡ "Electrification of Air by a Water Jet." By Magnus Maclean and Makita Goto, 'Phil. Mag.,' August, 1890, vol. 30, pp. 148—152.

The conditions under which the spectrum of argon has appeared in my investigations are of interest at the present time, and I hope a description of them may not be unacceptable.

A glass bulb was sealed hermetically to a mercury pump of the Hagen-Töpler form, in which there was strong sulphuric acid floating on the surface of the mercury. The bulb was exhausted as low as possible and refilled with air. The pressure was reduced to about 180 **M**illionths of an atmosphere (= 0·14 mm.), at which pressure a bright discharge could be passed through the residual gases by means of Professor J. J. Thomson's method of surrounding the bulb by a coil of wire, which carries a very rapidly alternating current produced by the discharge of a condenser.

The discharge was passed for thirty minutes, during which time a photograph of the spectrum was taken. The pressure of the gas in the bulb fell during the passage of the discharge from the value 174 **M** (= 0·13 mm.) to 112 **M** (= 0·085 mm.). The spectrum shows the bands of nitrogen strong, also mercury lines and nitrocarbon groups strong, hydrogen weak, no oxygen or argon.

Again the discharge was passed for thirty minutes and a new photograph was taken. The pressure fell from 100 M (= 0·076 mm.) to 20 M (= 0·015 mm.); the nitrogen spectrum had faded considerably, and there had appeared a number of fine lines which I was unable, in spite of careful efforts, to identify with the lines of any known substances.

The nature of my method of investigation of spectra is such that it is not difficult to pick out of the numerous spectra which appear superposed on the photographic plate, the lines which belong to any one spectrum. [The photographic reproductions sent herewith show clearly the ease with which this may be done.]

The results of measurement made in the last few days of seventy-two lines in my "low-pressure spectrum" are given below, and side by side are given the measurements of the wave-lengths determined by Mr. Crookes for the argon lines.

The agreement of the measurements shows conclusively that we have been measuring the same spectrum. Between H_γ and wave-length 370, the agreement is all that we could hope for, taking into account the fact that my measurements were not made with a view of giving a final and carefully-considered set of measurements of wave-lengths, but between H_γ and H_β there is a systematic difference of about 3 tenth-metres, which I am unable at present to account for. The agreement of grouping and intensity, however, leaves no doubt as to the identity of the spectrum of my low-pressure lines with the spectrum of argon. I have reduced my measurements with reference to Rowland's scale of wave-lengths, and I infer from the value adopted for the H_β (F) line, that Ångström's scale has been used in Mr. Crookes' reduction. The difference between the scales is not enough to account for the discrepancies above referred to.

The experiments were repeated, with slight variations, several times with results which, so far as the spectrum of argon is concerned, were constant. But it was noted that continued passage of the discharge appears to result in the attaining of a certain minimum pressure, after which there is slight and slow rise to a tolerably-fixed pressure. It is not necessary to dwell on these points in the present note.

It is interesting to find argon asserting itself, unsolicited, in quite new circumstances, and under conditions which practically constitute one more mode of separating argon from nitrogen—namely, the getting rid of nitrogen by passing electric discharge through it in the presence of hydrogen or moisture and acid.

Table of Wave-lengths.

H. F. Newall.		William Crookes. January 24, 1895. The Two Spectra of Argon.			
Measurements of lines on photograph.		Blue.		Red.	
Wave-length.	Intensity.	Wave-length.	Intensity.	Wave-length.	Intensity.
4879·8	5	487·9	10	487·9	4
4847·2	5	484·75	1		
4806·0	9	480·50	7		
4766·6	5	476·80	1		
4738·0	8	473·45	6		
4729·4	6	472·66	2		
				470·12	8
4659·6	7	465·65	5		
4644·0	1				
4639·0	2				
4632·1	4			462·95	5

Table of Wave-lengths—*continued.*

H. F. Newall.		William Crookes. January 24, 1895. The Two Spectra of Argon.			
Measurements of lines on photograph.		Blue.		Red.	
Wave-length.	Intensity.	Wave-length.	Intensity.	Wave-length.	Intensity.
4611·0	9	460·80	8		
4592·0	8			459·45	2
		458·69	6		
4581·2	6	457·95	6		
4546·5	7	454·35	7		
				451·40	2
		450·95	8	450·95	9
4482·2	6	447·83	6		
4460·0	2				
4431·3	10	442·65	10		
4426·0	10	442·25	10		
4421·2	4				
4414·1	4				
4401·7	9	439·95	10		
4400·1	5				
4379·8	8	437·65	9		
4375·8	3				
4370·4	8	436·90	9		
4351·4	7	434·85	10		
4336·0	2			434·50	5
4330·8	10	433·35	9	433·35	9
4308·7	4			430·05	9
4299·4	4	429·90	9		
4282·1	6				
4277·4	8 ? N	427·70	3		
		427·20	7	427·20	8
4266·4	9 ? N	426·60	6	426·60	4
		425·95	8	425·95	9
		425·15	2	425·15	3
4227·5	8	422·85	6		
		420·10	10	420·10	10
CN group has, though only of intensity 5, obliterated this set of lines.		419·80	9	419·80	9
		419·15	9	419·15	9
		418·30	8	418·30	8
		416·45	8	416·45	4
		415·95	10	415·95	10
4155·8				415·65	6
4130·9	6	413·15	3		
4104·2	8	410·50	8		
4082·2	4				
4075·8	3				
4072·4	9	407·25	8		
4069·7	2				
4042·7	5	404·40	8	404·40	9
4038·2	5				

Table of Wave-lengths—*continued.*

H. F. Newall.		William Crookes. January 24, 1895. The Two Spectra of Argon.			
Measurements of lines on photograph.		Blue.		Red.	
Wave-length.	Intensity.	Wave-length.	Intensity.	Wave-length.	Intensity.
4035·0	2				
4033·7	3	403·30	1		
4013·8	8	401·30	8		
3994·8	6				
3991·3	4				
3979·2	3	397·85	1		
3973·0	4				
3968·0	7	396·78	3		
		394·85	9	394·85	10
3944·1	5	394·35	3		
3932·3	5				
3930·8	3	393·18	3		
3928·2	8	392·85	9		
3920·3	6				
		392·75	3		
3918·8	5				
		391·50	1		
3892·2?	—			390·45	8
3883·2	5	389·20	5		
3873·4	4	387·55	2		
		387·18	2		
3868·1	6	386·85	8		
3850·8	7	385·15	10		
		384·55	1		
		383·55	2	383·55	8
3827·0	—	382·75	2		
3809·8	4	380·95	4		
		380·35	1		
		379·95	1		
3781·8	6	378·08	9	377·15	1
		377·05	2		
3766·1	5	376·60	8		
3750·2	3				
3738·8	3	373·85	3		
3730·0	8	372·98	10		
3719·2	2	371·80	4		

III. "Iron and Steel at Welding Temperatures." By THOMAS WRIGHTSON, Memb. Inst. C.E. Communicated by Professor ROBERTS-AUSTEN, C.B., F.R.S. Received February 2, 1895.

(Abstract.)

The object of this paper is to demonstrate that the phenomenon of welding in iron is identical with that of regelation in ice.

The author recapitulates experiments made by him in 1879–80, described in the proceedings of the Iron and Steel Institute for those years.

These experiments were upon cast iron, and proved the fact that this form of iron possessed the property of expanding while passing from the liquid to the plastic state during a small range of temperature, and then contracted to the solid state, and that the expansion amounted to about 6 per cent. in volume.

The experiments were carried out under two distinct methods, the first being by the suspending of a cast-iron ball on a spiral spring, and lowering the ball under the surface of a vessel filled with molten iron of the same quality; the change of volume was registered by the contraction of the spring as the varying displacement of the ball varied its buoyancy.

The second method was by casting 15-in. spheres of cast iron, and measuring the changing diameter as the sphere cooled, then laying down on paper a curve of changing volume, which in general character was found to be similar to the curves produced by the instrument used in the first method.

This property of iron resembles the similar property of water in freezing, which, within a range of about 4° C., expands about 9 per cent. of its liquid volume, and then contracts as the cooling proceeds.

This property of water was investigated by Professor James Thomson and by Lord Kelvin. The former showed that from theoretical considerations there was reason to expect that in the case of a body exhibiting the anomalous property of expanding when cooled and contracting when heated, it should be cooled instead of heated by pressure or impact.

Lord Kelvin investigated the problem experimentally as affecting freezing water, and completely demonstrated the truth of his brother's reasoning.

The experiments made by the author in 1879 and 1880 suggested the view that this property of ice was connected with the property of welding in iron, but this was only hypothetical, as the experiments had been made on cast iron, which, probably on account of the

presence of carbon, does not possess the property of welding. Further, it was not practicable to experiment with wrought iron in the same way as with cast iron on account of the difficulty of dealing with that substance in its liquid form.

Professor Roberts-Austen has, however, given metallurgical research a recording pyrometer, and this has enabled the author to resume the investigation at the Mint, where he had the advantage of Professor Roberts-Austen's assistance and advice. The method adopted was the heating of bars in an electric welder, and as soon as the junction of the bars was at a welding temperature, end pressure was applied by mechanical power, and the weld effected.

The temperature at the point of welding was observed by placing a thermo-junction at this point consisting of a platinum wire twisted into a second wire of platinum alloyed with 10 per cent. of rhodium. The electric current produced at the thermo-junction deflected a galvanometer, which by means of a mirror threw a spot of light upon a sensitised plate which moved by clockwork uniformly in a direction transverse to the spot of light. This produced a curve, the ordinates of which represented time and temperature.

These curves appear to show that a molecular lowering of temperature took place immediately the pressure was applied to the bar when in the welding condition.

Photographic curves are exhibited which show that this fall in temperature varied in these particular experiments from 57° C. to 19° C., according to the circumstances of temperature and pressure.

This appears to prove that wrought iron at a welding temperature possesses the same property of cooling under pressure which was proved by Lord Kelvin to exist in freezing water, and on which demonstration the generally received theory of regelation depends.

The author distinguishes the process of melting together of metals from that of welding.

Either process forms a junction, but the latter takes place at a temperature considerably below the melting point.

The well-known and useful property of welding in iron appears, therefore, to depend, as in the case of regelation in ice, upon this critical condition, which exists over a limited range of temperature between the molten and the plastic state.

IV. "The Composition of the Extinctive Atmospheres produced by Flames." By FRANK CLOWES, D.Sc. Lond. Communicated by Professor ARMSTRONG, F.R.S. Received February 7, 1895.

In a former paper ('Roy. Soc. Proc.,' vol. 56), the author communicated the results obtained by mingling gases, which were extinctive of flame, with air, until a flame burning in the air was just extinguished. The gases used in the experiments were carbon dioxide and nitrogen. Each of these gases was separately introduced into the air, and the composition of the atmosphere thus produced, which just extinguished flame, was determined by chemical analysis.

The general results arrived at were:—

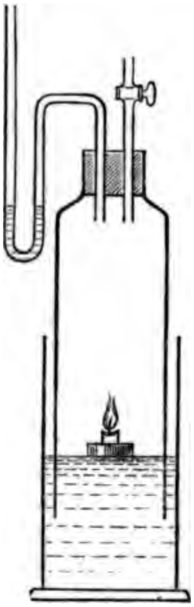
1. That wick-fed flames require atmospheres of very similar composition to extinguish them: while gas-fed flames require atmospheres of widely different composition.
2. That nitrogen must be added in larger proportion than carbon dioxide, in order to extinguish the same flame.
3. That the minimum proportion of extinctive gas which must be mingled with air in order to extinguish a flame is independent of the size of the flame.

A supplementary series of experiments has now been undertaken in order to determine the composition of the atmosphere extinctive of each flame, which is produced by the flame itself when burning in an inclosed volume of air at atmospheric pressure.

Preliminary trials showed that in order to secure trustworthy results, the atmosphere must not be in contact with water. It was further found necessary to avoid change of pressure in the inclosed atmosphere during the combustion of the flame.

These necessary conditions were complied with by allowing the flames to burn in a bell-jar, the bottom of which was closed by dipping into a sufficiently deep vessel of mercury. The neck of the bell-jar was closed by a cork bearing a U-shaped mercury gauge, and a glass tube with a stopcock.

While the flame was burning, the pressure of the air within the bell-jar was maintained uniform by raising or lowering the bell-jar sufficiently to maintain the mercury levels unchanged in the pressure gauge. As soon as the flame was extinguished by the atmosphere around it, the vessel was allowed to stand until the atmosphere within had become uniform in temperature with the air around. A portion of the gases within the bell-jar was then removed by means of the stopcock-tube to a Hempel gas-burette, and was subjected to ordinary



volumetric analysis by absorption. Each experiment and analysis was repeated in duplicate, and frequently in triplicate. The results obtained were either identical or closely concordant, except in the case of methane; but the flame of ethylene could not be maintained under the conditions of the experiment.

The gas was burnt from a tube passing through the mercury and terminating in a jet just above the surface of the mercury. The liquids were burnt from wicks contained in small shallow vessels floating upon the surface of the mercury.

As in the previous series of experiments (*loc. cit.*) the combustible substances used were chiefly those which are burnt for ordinary heating and lighting purposes.

The mean results are tabulated below.

Percentage Composition of the Residual Atmosphere and of the Artificial Atmosphere in which Flame is just extinguished.

Combustible substances burnt.	Percentage composition of the residual atmosphere in which the flame was extinguished.			Proportions per cent. of O_2 and N_2 in which flame is extinguished when introduced.	
	O_2.	N_2.	CO_2.	O_2.	N_2.
Alcohol, absolute	14·9	80·7	4·35	16·6	83·4
Alcohol, methylated	15·6	80·25	4·15	17·2	82·8
Paraffin, lamp oil	16·6	80·4	3·0	16·2	83·8
Colza and paraffin	16·4	80·5	3·1	16·4	83·6
Candle	15·7	81·1	3·2	16·4	83·6
Hydrogen	5·5	94·5	—	6·3	93·7
Carbon monoxide	13·35	74·4	12·25	15·1	84·9
Methane	15·6	82·1	2·3	17·4	82·6
Ethylene (failed)	—	—	—	[13·2	86·8]
Coal-gas	11·35	83·75	4·9	11·3	88·7

In the above table the results are given, which were obtained by the analysis of the residual atmospheres, in which the flame had burnt until it was extinguished. Since the proportions of carbon dioxide contained in these atmospheres are small, the composition of the artificial atmospheres consisting of nitrogen and oxygen only, in which the flames were just extinguished when they are inserted, are also stated for comparison. It will be noticed that the general agreement in composition shown by the two classes of extinctive atmospheres, so differently produced, is well maintained; and when the conditions of the experiments are taken into account the recent series of results are confirmatory of the general accuracy of those previously obtained.

I have to acknowledge the assistance rendered by M. E. Feilmann, B.Sc., in carrying out the experimental work involved in this investigation.

[February 18, 1895.] It is noteworthy that the composition of the extinctive atmosphere produced by the flame of a candle, or of oil, or of alcohol, closely corresponds with the average composition of air expired from the lungs. The composition of the last portion of air which is expired varies somewhat with the length of time during which the air has been retained in the lungs. The following percentage composition by volume was determined :—

Analyses of expired air.	O_2.	N_2.	CO_2.
Air expired immediately after having been inhaled	17·4	78·4	4·2
Air expired about 40 seconds after having been inhaled	14·9	81·4	3·7
Average composition of expired air	16·15	79·9	3·95

According to the statements published by Dr. J. Haldane ('Roy. Soc. Proc.,' December 6, 1894), an atmosphere of the average composition of expired air, or of that left by the combustion of candles or lamps, although it is extinctive of the flames of candles and of lamps, can be breathed by most people without producing any distinctly noticeable effect. With some people, the increased proportion of carbon dioxide would cause the inspirations to be somewhat deeper than usual, but in no case would injury to health result from breathing such an atmosphere.

The statement made by the author in a previous paper ('Roy. Soc. Proc.,' vol. 56), that men could apparently breathe with safety an atmosphere which just extinguished the flame of a candle or of a lamp, is therefore fully borne out by the above results. This agrees with the statements made by experienced mining authorities. Accordingly the extinction of such ordinary illuminating flames cannot be considered as proof that an atmosphere is not respirable with safety.

The residual atmosphere produced by the hydrogen flame is undoubtedly not respirable, on account of the greatly diminished proportion of oxygen which it contains. The colour of the hydrogen flame undergoes a distinct change in colour from reddish-purple to blue as the proportion of oxygen in the atmosphere is diminished. Since a similar change of colour is noticed when carbon dioxide is present in the atmosphere, it is probably due to the lowering of temperature of the flame: all attempts to detect the production of carbon monoxide by this flame, when it is burning in air containing carbon dioxide, have failed.

The conclusions which may be drawn from the above results are that:—

1. The flames of the combustible gases and liquids, which were experimented upon, produce, at the point of extinction in an enclosed atmosphere, a change in the proportion of oxygen in the air generally corresponding to that produced by preparing extinctive atmospheres by artificial mixture.
2. The flames of candles and lamps, when they are extinguished by

burning in a confined space of air, produce an atmosphere of almost identical composition with that of air expired from the lungs.

3. The extinctive atmospheres produced by the combustion of the flames of candles and of lamps, and the air expired from the lungs after inspiring fresh air, are respirable with safety.
4. The extinction of an ordinary candle or lamp flame is not necessarily indicative of the unsuitability of an atmosphere to maintain life when it is breathed.

Presents, February 21, 1895.

Transactions.

Calcutta:—Asiatic Society of Bengal. Journal. Vol. LXIII. Part 1. No. 3. Vol. LXIII. Part 2. No. 3. 8vo. *Calcutta* 1894; Proceedings. 1894. No. IX. 8vo. *Calcutta.*
The Society.

Chapel Hill, N.C.:—Elisha Mitchell Scientific Society. Journal. Vol. XI. Part 1. 8vo. *Chapel Hill, N.C.* 1894.
The Society.

Charlottenburg:—Königliche Technische Hochschule. Rede zum Geburtsfeste Seiner Majestät des Kaisers: Das Gesetz von der Erhaltung der Energie, &c. 8vo. *Berlin* 1895.
The School.

Jena:—Medicinisch-Naturwissenschaftliche Gesellschaft. Denkschriften. Bände 4, 5, und 8. Folio. *Jena* 1893–94; Jenaische Zeitschrift für Naturwissenschaft. Neue Folge. Band 22. Heft 2. 8vo. *Jena* 1894. The Society.

Kazan:—Imperial University. Scientific Notes. 1895. No. 1. [*Russian.*] 8vo. *Kazan.* The University.

Kew:—Royal Gardens. Bulletin of Miscellaneous Information. 1895. No. 97. 8vo. *London.* The Director.

Lausanne:—Société Vaudoise des Sciences Naturelles. Bulletin. 3e S. Vol. XXX. No. 115. 8vo. *Lausanne* 1894.
The Society.

London:—Anthropological Institute. Journal. Vol. XXIV. No. 3. 8vo. *London* 1895. The Institute.

British Association for the Advancement of Science. Report of the Meeting at Oxford, August, 1894. 8vo. *London* 1894.
The Association.

British Astronomical Association. Journal. Vol. V. No. 3. 8vo. *London* 1895. The Association.

Camera Club. Journal. Vol. IX. No. 105. 4to. *London* 1895.
The Club.

Transactions (*continued*).

Linnean Society. Journal (Botany). Vol. XXX. No. 210. 8vo. *London* 1895. The Society.

Physical Society. Proceedings. Vol. XIII. Part 3. 8vo. *London* 1895. The Society.

Royal United Service Institution. Journal. February, 1895. 8vo. *London.* The Institution.

Society of Antiquaries. Proceedings. Vol. XV. No. 2. 8vo. *London* 1894; Index of Archæological Papers published in 1893. 8vo. *London* 1894. The Society.

Plymouth:—Plymouth Institution and Devon and Cornwall Natural History Society. Transactions. Vol. XI. Part 4; and Annual Report 1893–94. 8vo. *Plymouth* 1894. The Institution.

Vienna:—K.K. Zoologisch-Botanische Gesellschaft. Verhandlungen. Band XLV. Heft 1. 8vo. *Wien* 1895. The Society.

Observations and Reports.

Bombay:—Archæological Survey of Western India. Progress Report. 1893–94. Folio. [*Bombay*] 1894. Government of Bombay.

Calcutta:—Meteorological Department of the Government of India. Indian Meteorological Memoirs. Vol. V. Part 6. Folio. *Calcutta* 1894. The Department.

Darlington:—Wolsingham Observatory. Report. 1894. 8vo. *Darlington* 1895. The Observatory.

India:—Archæological Survey. Epigraphia Indica. Vol. II. Part 16. Folio. *Calcutta* 1894. The Survey.

London:—Army Medical Department. Report, with Appendix. 1893. 8vo. *London* 1895. The War Office.

Middlesex Hospital. Reports of the Medical and Surgical Registrars and Pathologist. 1893. 8vo. *London* 1894. The Hospital.

Sydney:—Meteorological Observations. August, 1894. 8vo. [*Sydney*] 1894. The Observatory.

Victoria:—Public Library, Museums, and National Gallery. Descriptive Catalogue of the Specimens in the Industrial and Technological Museum, Melbourne, illustrating the Economic Woods of Victoria. 8vo. *Melbourne* 1894. The Public Library.

Washington:—Weather Bureau. Monthly Report of the Maryland State Weather Service, August, October, and November, 1894. 8vo. [*Baltimore*]; Report of the Ohio Weather and Crop Service. December, 1894. 8vo. *Columbus, Ohio* 1895. The Bureau.

Brongniart (Charles) Recherches pour servir à l'Histoire des Insectes Fossiles des Temps Primaires, précédées d'une Étude sur la Nervation des Ailes des Insectes. Texte & Atlas. 4to. *Saint Etienne* 1893. The Author, through Sir John Lubbock.

Donisthorpe (Wordsworth) Measures, Past, Present, and Future of Length, Area, Bulk, &c. 4to. *London* 1895. The Author.

Godman (F. Ducane), F.R.S., and Salvin (Osbert), F.R.S. Biologia Centrali-Americana; or Contributions to the Knowledge of the Fauna and Flora of Mexico and Central America. Zoology. Parts 1—120; Botany. 5 vols. 4to. *London* 1879–95. Messrs. Godman and Salvin.

Prince (C. Leeson) The Summary of a Meteorological Journal kept by the above at his Observatory, Crowborough Hill, Sussex. Folio. *Crowborough Hill* 1894. The Author.

Soria y Mata (Arturo) Origen Poliédrico de las Especies, Unidad, Origen, Reproducción y Sintesis de las Formas. 8vo. *Madrid* 1894. The Author.

Welch (Charles) History of the Tower Bridge and of other Bridges over the Thames built by the Corporation of London. 4to. *London* 1894. Bridge House Estates Committee.

February 28, 1895.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

Professor Alexander Agassiz, who was elected a Foreign Member in 1891, was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

This Meeting having been appointed by Council as a Meeting for Discussion, the following papers were taken as the subject of the discussion:—

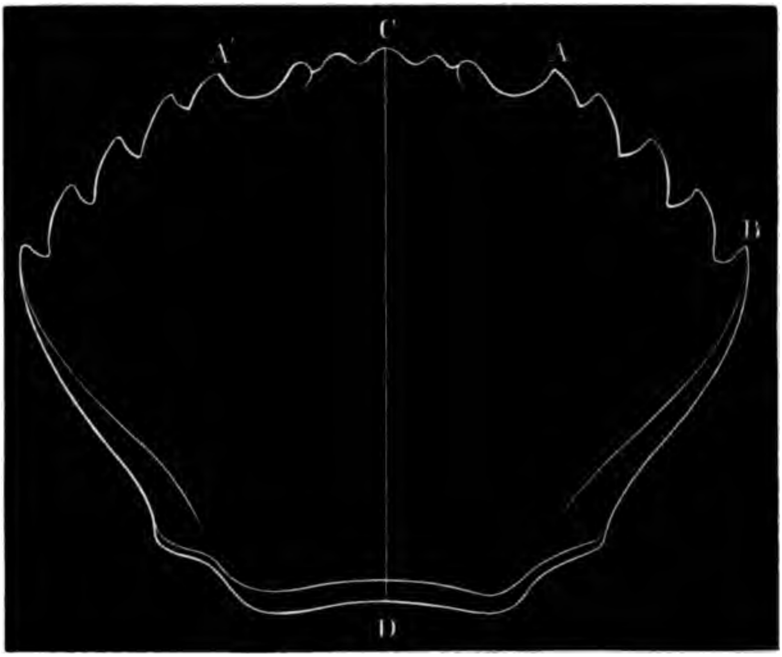
I. Report of the Committee, consisting of Mr. Galton (Chairman), Mr. F. Darwin, Professor Macalister, Professor Meldola, Professor Poulton, and Professor Weldon, "for Conducting Statistical Inquiries into the Measurable Characteristics of Plants and Animals." Part I. "An Attempt to Measure the Death-rate due to the Selective Destruction of *Carcinus Mænas* with respect to a Particular Dimension."—Drawn up for the Committee by Professor WELDON, F.R.S. Received November 20, 1894.

Among the material available for the purposes of the Committee was a sample of *Carcinus mænas*, from Plymouth Sound, including a fairly large number of young females. The distribution of abnormalities in certain dimensions had already been determined for adult females from the same locality ('Roy. Soc. Proc.,' vol. 54, pp. 318—329); and it seemed worth while to compare the frequency of abnormalities in young individuals at various stages of growth with the frequency of the same abnormalities in adult life, so as to determine whether any evidence of selective destruction during growth could be discovered or not.

About 7000 females, varying in length from 7·00 to 13·95 mm., were chosen (at random, except as regards their size), and two dimensions were measured in each. The results were then compared with those of the corresponding measurements, made upon a sample of 1000 adult females from the same locality, which are recorded in the paper just referred to.

The dimensions chosen were:—(1) the "*frontal breadth*"—the distance in a straight line between the tips of the extra-orbital teeth of the carapace (from the point A, fig. 1, to the corresponding point on the opposite side); and (2) the "*right dentary margin*," measured in a straight line from the apex of the first to that of the last lateral tooth (from A to B, fig. 1). The "length" of each crab was taken as the length of the carapace, from the tip of the middle inter-orbital tooth to the posterior margin (from C to D, fig. 1). This is, of course, not the total length of the body; but the curvature and flexibility of the abdomen render an exact determination of the real body length very difficult.

FIG. 1.



In order to compare the variability of a dimension in crabs whose carapace is only 7 mm. long with that of the corresponding dimension in adult crabs, whose carapace length is from 40—50 mm. or more, it is evidently necessary to adopt some method of picturing the crabs as of one standard size; and accordingly the measures obtained have always been expressed in terms of the carapace-length of the crab to which they belong, taken as 1000. The measurements were made by means of a screw, of 1 mm. pitch, carrying the object across the field of a microscope, and by means of graduations on the head of the screw the observations were recorded to the nearest hundredth of a millimetre. It is believed that the probable error of any observation is not much more than one hundredth of a millimetre. In order to minimise the effect of errors of observation, the results, after being expressed as fractions of the carapace-length, were sorted into groups, such that the measures in each group did not differ by more than 0·004 of the carapace-length, and all measures in the same group were treated as identical. The unit employed in tabulating the results was therefore 0·004 of the carapace-length; but in what follows the results are expressed, for the greater convenience of the reader, in thousandths of the carapace-length. It will be noticed that the principal effect of this alteration upon the results is to diminish their apparent regularity—an aberration of one unit of measurement appearing as four units in the tables below.

1. *Variation in Frontal Breadth.*

An initial difficulty in determining the error of distribution of frontal breadths about their mean in young crabs, arises from the great rapidity with which the mean itself changes during growth. The mean frontal breadth in the smallest specimens was found to be 853·14 thousandths of the carapace-length, while at maturity it is only 604·94 thousandths. The rate at which this change occurs can be gathered from the following table of the crabs measured, and the same result is graphically shown in fig. 2.

From this table it appears that the mean frontal breadth changes at such a rate that when the carapace-length has increased 0·2 mm., the frontal breadth has almost always diminished by less than four thousandths, that is to say, by less than one of the units of measurement here employed. For the purpose of the present investigation the mean was therefore considered stationary during every period of increase in size of not more than 0·2 mm., and the young crabs were accordingly sorted into groups, the individuals of each group differing by less than 0·2 mm. in respect of their carapace-length. The distribution of frontal breadths about the mean was then examined in each group separately.

As the difference in size between the largest and the smallest of the growing crabs was 7 mm., it follows that the material was divided into thirty-five groups. This subdivision of the material had great disadvantages, because, instead of a single group of over 7000 individuals, varying about the same mean, from which a fairly reliable indication of the law governing frequency of deviation might have been expected, the average number of individuals in any one of the available groups was only 200; and from so small a number of obser-

Table I.—Mean Frontal Breadth (F) expressed in thousandths of the Carapace-length, corresponding to various Carapace-lengths (C), together with the Number of Individuals on which each Determination is based.

C.	F.	Number.	C.	F.	Number.
7·1	853·14	159	10·7	798·01	225
7·3	852·43	186	10·9	794·96	162
7·5	850·89	172	11·1	792·14	222
7·7	844·27	142	11·3	789·26	218
7·9	844·22	132	11·5	789·26	230
8·1	837·13	224	11·7	786·07	211
8·3	835·41	219	11·9	784·53	225
8·5	830·08	214	12·1	782·42	224
8·7	826·80	207	12·3	780·92	226
8·9	823·75	214	12·5	778·39	219
9·1	821·26	191	12·7	772·76	183
9·3	818·33	205	12·9	771·62	233
9·5	815·89	214	13·1	770·36	131
9·7	811·60	195	13·3	769·86	162
9·9	809·95	226	13·5	767·70	158
10·1	809·27	245	13·7	762·51	201
10·3	803·21	253	13·9	763·47	211
10·5	800·53	232	(Adult)	(604·94)	(998)

[*Note.*—The carapace-length given in the table is the mean of all lengths included in each group. For example, the entry 7·1 includes all crabs measured in which the carapace-length was between 7·00 mm. and 7·19 mm., and so on.]

vations no satisfactory demonstration of the law of variation at any given moment of growth could be obtained. Nevertheless it was necessary, before proceeding further, to ascertain with some certainty what the law of variation through the whole series really was. The belief in which the work was undertaken was, that the law of variation would be found throughout to be that of the ordinary probability equation; and this belief was tested in the following way:—In each of the thirty-five groups, the arithmetic mean of the frontal breadths, and the mean of all the deviations from it, were determined; and from the "mean error" found in this way the modulus of the probability function was calculated. Then, by calling the mean of each group zero, and expressing the deviations from the mean in terms of the modulus, a number of curves were obtained, in each of which the modulus was unity and the mean zero; a similar curve of adults was constructed, and the corresponding ordinates of all the thirty-six curves so obtained were added together. It is evident that, if the chance function really expresses the law of variation throughout the series, then the curve resulting from the treatment described will be a symmetrical probability curve of unit modulus. The actual

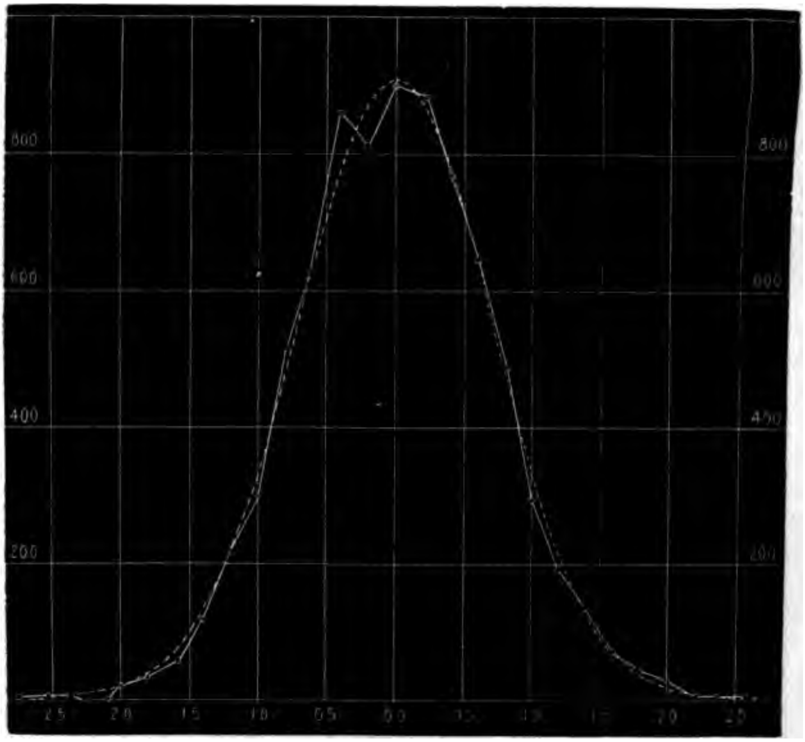


Fig. 3.—Distribution of Frontal Breadths in 8069 Female Crabs from Plymouth Sound, old and young. Deviations expressed in terms of the Modulus. The three cases of deviation greater than three times the Modulus are omitted.

deviations and that indicated by the probability integral is fairly close. The mean error of the observed curve is 0·5621, whereas it should be $1/\sqrt{\pi} = 0\cdot5642$, the difference between the two figures being less than 0·5 per cent. The error of mean square is 0·7123, instead of 0·7071, a difference of less than 1 per cent. The sum of the squares of the positive deviations is 2115. The sum of the negative deviations is 1992. The total number of individuals of deviation more than 0·1 is 3593 on the positive, 3574 on the negative side, a difference of about one-half per cent.

On the whole it may be said that the result agrees with that given by the theory of probability as well as could be expected from the number of observations, and that the law of frequency of variation throughout the series may, as was hoped, be assumed to agree with the ordinary law of chance.

From the result so far obtained it followed that a determination of the quartile deviation, or any other of the constants of the pro-

Table II.—Frequency of all Observed Deviations from the Mean Frontal Breadth in 8069 Female Crabs, young and adult, from Plymouth. The Deviations expressed in terms of the Modulus.

Limits of deviations.	Mean deviation.	Observed frequency.	Theoretical frequency.
Over +3·29	+4·790	2	12
From +3·10 to 3·29	+3·280	1	
„ +2·90 „ 3·09	—	0	
„ +2·70 „ 2·88	+2·880	1	
„ +2·50 „ 2·69	+2·575	4	
„ +2·30 „ 2·49	+2·355	4	
„ +2·10 „ 2·29	+2·207	4	
„ +1·90 „ 2·09	+2·003	28	17
„ +1·70 „ 1·89	+1·788	46	36
„ +1·50 „ 1·69	+1·598	71	71
„ +1·30 „ 1·49	+1·395	137	129
„ +1·10 „ 1·29	+1·205	194	217
„ +0·90 „ 1·09	+1·006	295	336
„ +0·70 „ 0·89	+0·805	492	481
„ +0·50 „ 0·69	+0·589	649	635
„ +0·30 „ 0·49	+0·391	769	774
„ +0·10 „ 0·29	+0·191	896	872
„ +0·09 „ −0·09	−0·011	902	907
„ −0·10 „ 0·29	−0·213	814	872
„ −0·30 „ 0·49	−0·404	862	774
„ −0·50 „ 0·69	−0·611	626	635
„ −0·70 „ 0·89	−0·806	517	481
„ −0·90 „ 1·09	−1·000	299	336
„ −1·10 „ 1·29	−1·194	219	217
„ −1·30 „ 1·49	−1·403	118	129
„ −1·50 „ 1·69	−1·589	58	71
„ −1·70 „ 1·89	−1·808	33	36
„ −1·90 „ 2·09	−2·016	18	17
„ −2·10 „ 2·29	−2·210	1	12
„ −2·30 „ 2·49	−2·368	4	
„ −2·50 „ 2·69	−2·520	3	
„ −2·70 „ 2·89	−2·750	1	
—	—	—	
Over	−4·450	1	

bability equation, would be a trustworthy guide to the frequency of abnormalities at various periods of growth. But just as the individual groups were too small to allow of a determination of the law of abnormality in each, so they were too small to give trustworthy values of the quartile. The quartile deviation changes so slowly with growth, that it may without serious error be assumed to be constant during the period represented by 1 mm. of growth in carapace-length: that is, through the period covered by five of the groups into which the growing crabs were sorted. Therefore, after the quartile deviation had been determined in every group, the results were arranged in fives, and the mean of every consecutive five was taken as the quartile deviation through 1 mm. of growth. The results are shown in Table III.

Table III.—Quartile Deviation of Frontal Breadths (Q) for various Magnitudes of Carapace-length (C).

C.	Mean Q.
7·5	9·42
8·5	9·83*
9·5	9·51
10·5	9·58
11·5	10·25
12·5	10·79
13·5	10·09
(Adult)	(9·96)

The values here given are probably not very reliable, but they show that in the youngest individuals the quartile deviation is distinctly less than at maturity; that it increases with increase of size, until a time arrives when it is distinctly greater than in adult life; and that finally it diminishes again.

The initial features of this result,—the smallness of the quartile error at a young age, indicating relative infrequency of deviations, and the increase during growth, have been observed by Bowditch in the case of human stature. The result obtained by Dr. Bowditch and that here described are both simply confirmations of Darwin's statement, that many variations appear at a late period of development.

The initial increase in the quartile error may be attributed to the fact that average young produce upon the whole average adults, while animals which exhibit a deviation of known amount in the young state, exhibit on the whole a greater deviation with advancing age. If this view be the true one (and it is hoped that next year it may be possible to test it by observation of living crabs, which can be measured at various periods of growth), then, in a Plymouth crab, which is of unit deviation when its carapace is 7 mm. long, the most probable deviation when it has grown to be 12·5 mm. in length will be 10·79/9·42 = about 1·15 units. The probable error of this expectation is the expression of irregularities in the rate of growth, which cannot at present, for want of knowledge, be adequately discussed.

From the age represented by a length of 12·5 mm., the quartile error diminishes, and the parallel between the behaviour of the frontal breadth in Plymouth crabs and that seen by Bowditch in the stature of civilised human beings ceases to hold. The obvious suggestion by which to account for this seems to be that in the United States, where Bowditch made his observations, human beings are under conditions of such civilisation that there is considerable protection of the physically unfit; and that here, as in other civilised countries, any influences which might in a savage race produce selective destruction are reduced to a minimum, whereas in the case of the crabs such selective influences are active.

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* Of the four very abnormal values shown in the table, three occurred in this group. They have been omitted in the determination of the quartile deviation, which would otherwise become 9·92.

It is, of course, possible that the deviation of "abnormal" young may in each individual case first attain a maximum and then diminish with advancing age; if this is the case, we cannot know without experiment. In the absence of such experiment, the hypothesis may be provisionally adopted that the diminution in the frequency of individuals of given deviation is due to a selective destruction, and the consequences of this hypothesis will be examined.

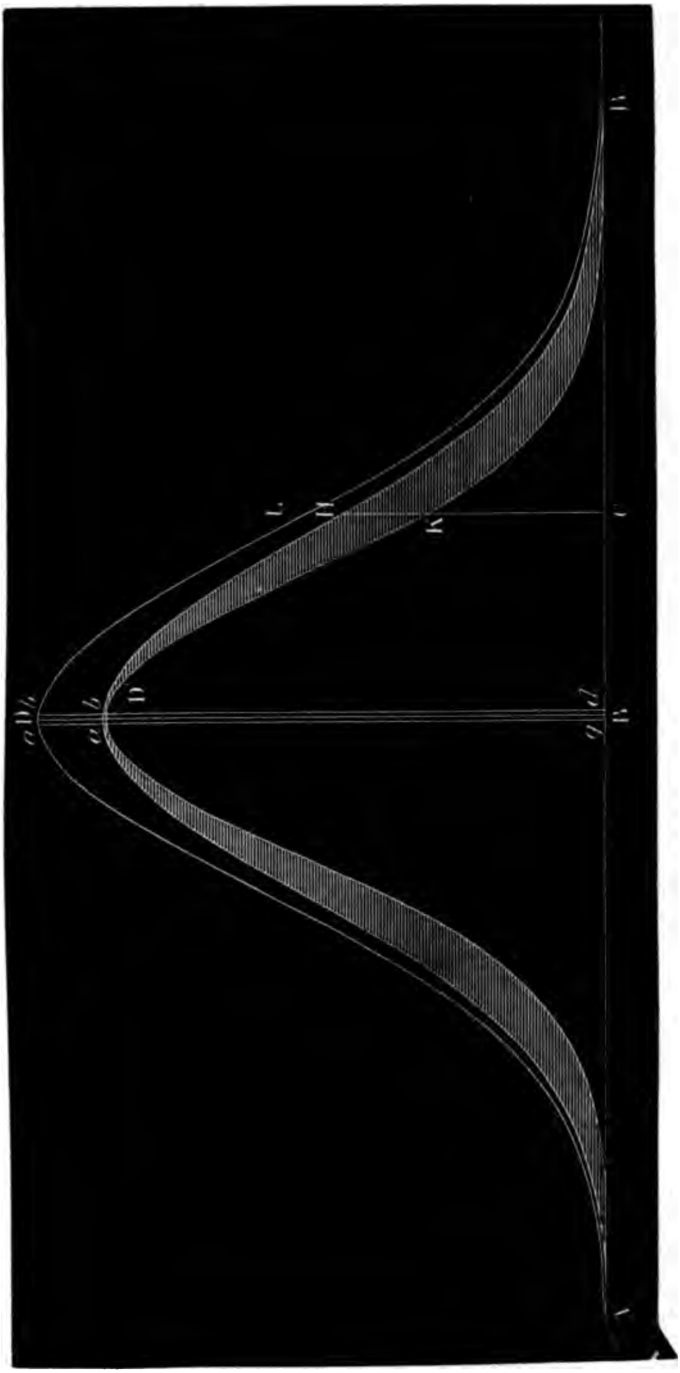
Consider a population of crabs, measured at the time of their maximum variability, and suppose the distribution of deviations among the population to be accurately represented, for a particular organ, by a probability equation of modulus c_1. Now, let the number of individuals of deviation lying between $\pm a$ be represented by the area *abgd* (fig. 4); then, if $gd = 2a$ be small, compared with the observed range of variation, and $k_1 = \frac{\text{area } abgd}{2a}$, in other words, if k_1 be the height of the median ordinate BD of the generalised curve, then the whole number of individuals in the population will be $k_1 c_1 \sqrt{\pi}$.

Now, suppose any destruction, which acts unselectively with regard to the organ in question, to reduce the number of individuals whose deviation lies between $\pm a$, to *cdef*, and let the $\frac{\text{area } cdef}{2a} = k_2$, or the height of the median ordinate BD^1. Since this destruction is unselective, it will destroy an equal percentage of animals of every deviation, and will therefore not alter the modulus. The population will therefore be reduced to $k_2 c_1 \sqrt{\pi}$ in number. This unselective destruction cannot be directly measured.

The selective destruction is most simply conceived as follows:—

In fig. 4 let AD^1HE represent a curve of modulus c_1, and let $BD^1 = k_2$, so that the area of the whole curve $AD^1HE = k_2 c_1 \sqrt{\pi}$ represents the population left after unselective destruction has occurred. Then suppose the modulus to be reduced during growth to c_2, where c_2 is less than c_1, and let AD^1KE be a curve of modulus c_2. The minimum number of individuals which it is necessary to destroy, in order to affect this reduction in the modulus, is evidently represented by the

FIG. 4.



shaded area of the figure. The population after such destruction is $k_2c_2\sqrt{\pi}$ in number, and the shaded area represents a number of individuals equal to $k_2\sqrt{\pi}(c_1 - c_2)$, so that the ratio of animals selectively destroyed to animals which survive all unselective destruction is $\frac{c_1 - c_2}{c_1}$, a quantity which can be experimentally determined.

From the data given in Table III, this ratio, for frontal breadths of Plymouth crabs, becomes $\frac{10{\cdot}79 - 9{\cdot}96}{10{\cdot}79}$ or about 0·077, so that the hypothesis of selective destruction involves a death-rate of about 77 per thousand between the age corresponding to 12·5 mm. in carapace-length and maturity, as a consequence of deviation in frontal breadths, and in the group of structures, whatever these may be, which are directly correlated with it.

This total death-rate does not affect individuals of all deviations alike; an inspection of the figure will show that the death-rate is a function of the deviation, and that function is quite simply determined. Consider any ordinate Eg of the curve ABEC, and let its abscissa, DG, be of magnitude x; then the length of Eg is $k_2e^{-x^2/c_1^2}$ and the number of individuals of deviation between x and $x + dx$ is $k_2e^{-x^2/c^2}.\,dx$. In the same way, the height of the ordinate Fg is

$$k_2e^{-x^2/c_2^2},$$

and the number of individuals of abnormality within unit distance of x after selection is

$$k_2e^{-x^2/c_2^2}\,.\,dx.$$

The ratio between the number of animals of abnormality x which survive the unselective destruction and those which are selectively destroyed is therefore

$$\frac{\mathrm{E}g-\mathrm{EF}}{\mathrm{E}g} = \frac{e^{-x^2/c_1^2}-e^{-x^2/c_2^2}}{e^{-x^2/c_1^2}} = 1 - e^{x^2(c_2^2-c_1^2)/c_1^2c_2^2}$$

So that if g is the selective death-rate among animals of abnormality x, then that death-rate increases as x increases according to the equation

$$g = 1-e^{-hx^2},$$

where h is the numerical value of $c_1^2-c_2^2/c_1^2c_2^2$, c_1 and c_2 being the values of the modulus at the time of its maximum value and at maturity respectively.

For the frontal breadth of Plymouth crabs, the value of h is about 0·015; so that of the whole number of animals which attain the size 12·5 mm., having an abnormality x of their frontal breadth, the

fraction destroyed as a consequence of this abnormality before reaching maturity is

$$1-e^{-0\cdot015x^2}.$$

It will, of course, be understood that little trust can be placed in the absolute numerical results which are here put forward; the point which seems worthy of confidence, and which if it be indeed a reality is of very great importance, is the form of the result. For by purely

ERRATA.

PROCEEDINGS, VOL. LVII.

On page 369, in the figure, *for* the right-hand letter B, *read* E.
On page 370, *for* E*g* *read* HC.
" " ABEC " AD′HE.
" " DG " BC.
" " F*g* " KC.
" " EF " HK.

not necessarily the position of minimum destruction. The treatment of this case requires the help of a professional mathematician.

It will be well to mention here a curious indirect confirmation of the result just described, based on evidence derived from a quite different source.

An attempt has been made to show that physiological accidents of a kind leading to change in the length of a portion of the carapace affect a crab at a rate measured by the value of the quantity $1-e^{-kx^2}$. The symmetrical distribution of variations from the mean which has been shown, especially by Mr. Galton, to occur in dimensions of weight, length, muscular strength, and other characters of various

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It will, of course, be understood that little trust can be placed in the absolute numerical results which are here put forward; the point which seems worthy of confidence, and which if it be indeed a reality is of very great importance, is the form of the result. For by purely statistical methods, without making any assumption as to the functional importance of the frontal breadth, the time of life at which natural selection must be assumed to act, if it acts at all, has been determined, and the selective death-rate has been exhibited as a function of the abnormality, while a numerical estimate which is at least of the same order as the amount of the selective destruction has been obtained.

The method by which the result described has been arrived at is likely to be capable of application to a very considerable number of cases. Mathematically considered, the conditions which were at first assumed and then proved to obtain in the organ discussed are by no means general. It is necessary for the employment of this method that the variations should be distributed on each side of the mean with sensible symmetry, and that the position of minimum selective destruction should be sensibly coincident with the mean of the whole system. Such statistical information as is at present available leads to the belief that these conditions may be expected to hold for a large number of species, which are sensibly in equilibrium with their present surroundings, so that their mean character is sensibly the best, and the change of mean from generation to generation is at least very small. They cannot be expected to hold in cases of rapid change such as those induced artificially by selection under domestication, or naturally by rapid migration or other phenomena resulting in a rapid change of environment.

For the investigation of such rapid change, it would be necessary to treat the more general case, in which the chances of deviations of opposite sign are not sensibly symmetrical, and in which the mean is not necessarily the position of minimum destruction. The treatment of this case requires the help of a professional mathematician.

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An attempt has been made to show that physiological accidents of a kind leading to change in the length of a portion of the carapace affect a crab at a rate measured by the value of the quantity $1-e^{-kx^2}$. The symmetrical distribution of variations from the mean which has been shown, especially by Mr. Galton, to occur in dimensions of weight, length, muscular strength, and other characters of various

organs in men, moths, sweet peas, and other things at various periods of life, made it seem probable that if selective destruction could be shown to occur in these cases, the expression for intensity of destruction, in terms of the deviation, would in all these cases be of the same form as that already arrived at. That is to say, the expression for the effect of physiological accidents of a number of different kinds, affecting a number of organisms in no way specially alike, is probably always of the same form. The question at once occurred, whether this expression might not be of general application, as a measure of the effect of physiological accidents upon the animal body.

The most convenient case in which to look for an answer to this question is the case of muscular tissue, in which the effect of accidents of stimulus can be readily measured. The recent paper of Cyboulski and Zanietowski (Pflüger's 'Archiv f. Physiologie,' Bd. 56, p. 45) gives an excellent series of data for determining the relation between energy of stimulus applied to a nerve, and effect upon the muscle, as measured by energy of external work performed in contraction. These observers give a large series of tables, in which the energy of stimulus, applied by discharging a condenser of known electrical capacity through a nerve, is given in one column, and in another is the work done by the muscle stimulated, measured by the height through which a known mass is lifted.

As is well known, the application of stimuli of less than a certain magnitude produces no muscular contraction; but if the maximum stimulus which can be applied without causing a contraction be reckoned as zero, the subsequent relation between stimulus and contraction does, in fact, agree very closely with that indicated by successive values of the quantity $1-e^{-kx^2}$.

In spite of the evident care and skill with which Cyboulski and Zanietowski have performed their experiments, their curves are slightly irregular. In order to minimise the effect of these slight irregularities, three of their results were treated in the following way:—In each system of observations the maximum subliminal stimulus was subtracted from the magnitude of the applied stimulus in each case; the three numbers representing the height of the muscle contraction for unit stimulus beyond this point in the three cases were added together; and so on throughout. The result is plotted in fig. 5, the height of the sum of three contractions being indicated by the ordinates of the points ⊙ ; the intensity of the corresponding stimulus, *minus* the subliminal stimulus, being measured along the abscissa.

The dotted curve is given by

$$g = 1-e^{-x^2/(8\cdot 15)^2},$$

Table V.—Frequency of all observed Deviations from the Mean Length of the Right Dentary Margin in 8020 Female Crabs, Young and Adult, from Plymouth. Deviations in terms of the Modulus.

Limits of deviations.	Mean deviation.	Observed frequency.	Theoretical frequency.
Over +3·20	+5·530	1	2·76
	+3·760	1	
	+3·340	1	
From +3·00 to 3·19	+3·120	1	
„ +2·80 „ 2·99	+2·945	2	
„ +2·60 „ 2·79	+2·677	3	
„ +2·40 „ 2·59	+2·493	3	
„ +2·20 „ 2·39	+2·253	12	4·71
„ +2·00 „ 2·19	+2·050	9	11·29
„ +1·80 „ 1·99	+1·888	16	24·99
„ +1·60 „ 1·79	+1·705	45	51·14
„ +1·40 „ 1·59	+1·492	132	96·49
„ +1·20 „ 1·39	+1·276	155	168·30
„ +1·00 „ 1·19	+1·108	238	271·13
„ +0·80 „ 0·99	+0·897	375	403·41
„ +0·60 „ 0·79	+0·700	587	554·36
„ +0·40 „ 0·59	+0·493	735	703·61
„ +0·20 „ 0·39	+0·305	775	824·82
„ +0·00 „ 0·19	+0·112	939	893·04
„ −0·00 „ 0·19	−0·088	871	893·04
„ −0·20 „ 0·39	−0·270	833	824·82
„ −0·40 „ 0·59	−0·495	698	703·61
„ −0·60 „ 0·79	−0·739	553	554·36
„ −0·80 „ 0·99	−0·896	446	403·41
„ −1·00 „ 1·19	−1·093	240	271·13
„ −1·20 „ 1·39	−1·279	162	168·30
„ −1·40 „ 1·59	−1·479	89	96·49
„ −1·60 „ 1·79	−1·691	45	51·14
„ −1·80 „ 1·99	−1·932	18	24·99
„ −2·00 „ 2·19	−2·108	15	11·29
„ −2·20 „ 2·39	−2·295	6	4·71
„ −2·90 „ 2·59	−2·487	3	2·76
„ −2·60 „ 2·79	−2·710	2	
„ −2·80 „ 2·99	−2·903	3	
Over 3·00 ..	−3·330	2	
	−4·180	1	
	−4·830	1	
	−5·960	1	
	−7·030	1	

The symmetry of these results is fairly good, the number of positively abnormal individuals being 4030, the number of negatively abnormal 3990. The sum of the squares of the negative deviations is 2145·5; the sum of the squares of the positive deviations being 2099·9—a difference of about 2 per cent. This difference is greater than in the case of the frontal breadths; but a reference to the table will show that the removal of a single individual, namely, the indi-

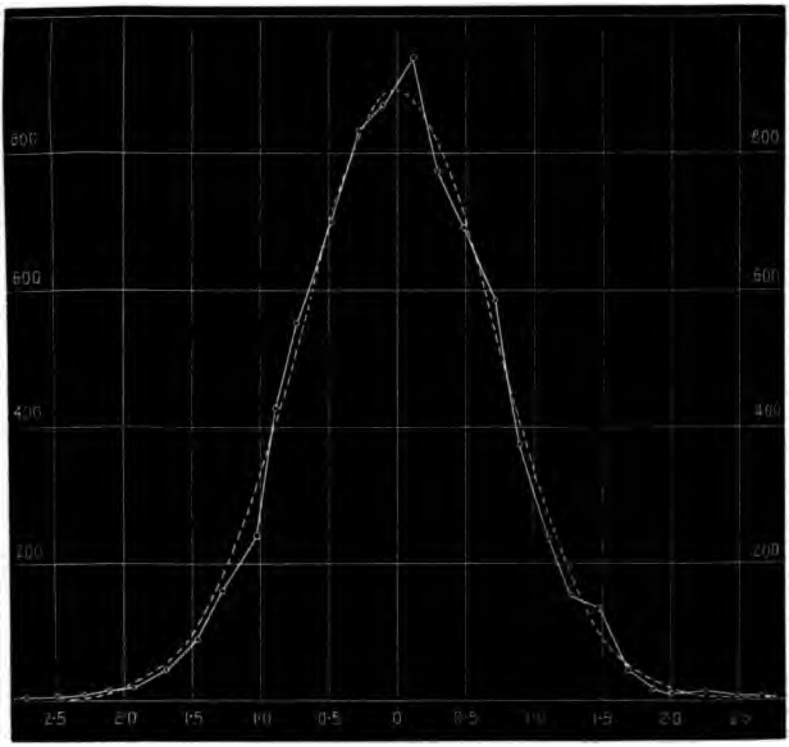


Fig. 6.—Deviations of 8020 measures of Right Dentary Margin in Female Crabs, old and young, from Plymouth Sound, expressed in terms of the Modulus. Eleven individuals of deviation greater than three times the Modulus are omitted.

vidual of deviation equal to −7 times the modulus, would make the sum of the positive and negative squares almost exactly equal.

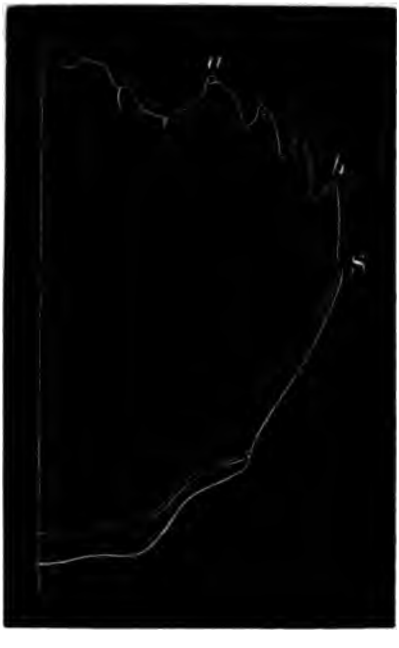
The mean error of the whole system is 0·5688 instead of 0·5642, or nearly 1 per cent. too great. The error of mean squares is 0·7276 instead of 0·7077, or 2·8 per cent. too great.

From these values of the mean error and error of mean square, as well as from the presence of a deviation so great as seven times the modulus, it is evident that some cause has been at work, producing large abnormalities with a frequency greater than that indicated by the theory of chance. Reference to the table shows that deviations of more that 2·5 times the modulus do in fact occur twenty times, instead of five or six times, as they should do So that deviations of this magnitude occur about three and a half times too often.

The sporadic occurrence of considerable deviations, which do not obey the general law of frequency of variation, is a phenomenon which has been supposed by many naturalists to be of great importance in evolution, and the present case is therefore worthy of discussion.

The following suggestion is offered as an explanation of the large *negative* deviations. As shown in Fig. 1, there are normally five teeth in the dentary margin; but occasionally (in over 1 per cent. of individuals) only four teeth occur. The reduction in teeth may apparently be effected in various ways: sometimes it is impossible to say that one tooth rather than another is missing; and the case then resembles those cases of variation in the segmentation of a vertebral column, for example, recently discussed by Bateson ("Materials for the Study of Variation," *passim*, especially, however, p. 124). In other cases, the reduction appears to be effected by a process resembling the filling up of the interval between two teeth, so that the points of the teeth project only very slightly. A careful outline of a specimen exhibiting this condition is given in fig. 7. It is evident that

Fig. 7.



in this case the little tubercle S which indicates the position of the fifth tooth is the point from which the measurement should be taken; but if the obliteration of the fifth tooth had progressed but a little further, no indication of its presence would remain; and the dentary margin, measured from the tip of the first to the tip of the last visible tooth, would really have been measured from *a* to *b*. Such a measure would not be homologous with the rest, and ought not to be included in the scheme. But, since reduction in the number of teeth may take place in other ways, and since it is impossible in a given case to distinguish the manner in which it has occurred, the measurements were necessarily recorded.

Another frequent cause of disturbance is the breakage of the last tooth, followed by its regeneration. All cases of obviously recent injury were of course excluded; and for this reason the total number of individuals discussed is reduced from 8069 to 8020. But the selection of material was felt to be so dangerous a proceeding that all cases in which there was any doubt as to the occurrence of an injury were included. The wrongful inclusion of a dozen such cases would account for the excess of positive abnormalities: for it is evident that a breakage of the tip of the last tooth would increase the distance AB in fig. 1.

While, therefore, the observations admit of the interpretation that about once in 500 cases a "sport" of magnitude greater than that provided for by the theory of chance does regularly occur, the considerations which have been submitted make this interpretation at least doubtful.

The value of the probable error, as an indication of percentage abnormality, is diminished by the existence of the large deviations discussed; but the values obtained are of considerable interest: they are as follows:—

Table VI.—Mean Value of Quartile Deviation (Q) of Right Dentary Margin for various Lengths of Carapace (C).

C.	Mean Q.
7·5	8·44
8·5	8·08
9·5	9·36
10·5	8·23
11·5	8·16
12·5	8·05
13·5	8·68
(Adult)	(9·28)

It will be seen from this table that the error of distribution at the ages measured is always less than in adult life, except among crabs, whose carapace length is between 9 and 10 mm. Of the fourteen superfluous deviations of great magnitude, three occur in this group, and the result is a quite untrustworthy determination.

Evidently, therefore, in spite of the abnormally great frequency with which large deviations occur, the whole percentage of abnormalities, among crabs between 7 and 14 mm. in length, is less than it is in adult crabs; and there is a rough agreement between the result obtained from these measurements and that obtained by Bowditch from the measurements of human stature already referred to. So that among female crabs in Plymouth Sound, during the period of life to which these observations refer, there is no indication of any destructive agency which acts selectively upon the dentary margin. Whether such selective destruction occurs among males, or among females at a later period of life, is for the present an open question.

Variation in frontal breadth may, therefore, for the present be considered to be of more importance in the economy of female crabs than variation in the length of the dentary margin—a view which receives confirmation from the dimorphism already shown to exist ('Roy. Soc. Proc.,' vol. 54, p. 324) in the frontal breadth of crabs from Naples, while it is a striking justification of the accepted system of classification, in which the characters of the great groups into which the Brachyura are divided are almost entirely those associated with changes in this dimension.

In conclusion, an important feature of the method employed may be pointed out. The increase of death-rate, associated with a given abnormality of frontal breadth, has here been roughly determined; in the previous paper, already referred to, the effect of abnormality in this dimension upon several other organs of the body was determined; and by the method of that paper it would be possible to determine the effect of parental abnormality upon the offspring. These are all the data which are necessary, in order to determine the direction and rate of evolution; and they may be obtained without introducing any theory of the physiological function of the organs investigated. The advantage of eliminating from the problem of evolution ideas which must often, from the nature of the case, rest chiefly upon guess-work, need hardly be insisted upon.

II. "Remarks on Variation in Animals and Plants. To accompany the first Report of the Committee for conducting Statistical Inquiries into the Measurable Characteristics of Plants and Animals." By Professor W. F. R. WELDON, F.R.S. Received February 19, 1895.

1. The importance of variation as a factor in organic evolution is not seriously disputed; but, if one may judge from the expressions contained in recent essays, naturalists are not agreed as to the

shaded area of the figure. The population after such destruction is $k_2c_2\sqrt{\pi}$ in number, and the shaded area represents a number of individuals equal to $k_2\sqrt{\pi}(c_1 - c_2)$, so that the ratio of animals selectively destroyed to animals which survive all unselective destruction is $\frac{c_1 - c_2}{c_1}$, a quantity which can be experimentally determined.

From the data given in Table III, this ratio, for frontal breadths

So that if g is the selective death-rate among animals of abnormality x, then that death-rate increases as x increases according to the equation

$$g = 1 - e^{-hx^2},$$

where h is the numerical value of $c_1^2 - c_2^2/c_1^2c_2^2$, c_1 and c_2 being the values of the modulus at the time of its maximum value and at maturity respectively.

For the frontal breadth of Plymouth crabs, the value of h is about 0·015; so that of the whole number of animals which attain the size 12·5 mm., having an abnormality x of their frontal breadth, the

fraction destroyed as a consequence of this abnormality before reaching maturity is

$$1-e^{-0{\cdot}015x^2}.$$

It will, of course, be understood that little trust can be placed in the absolute numerical results which are here put forward; the point which seems worthy of confidence, and which if it be indeed a reality is of very great importance, is the form of the result. For by purely statistical methods, without making any assumption as to the functional importance of the frontal breadth, the time of life at which natural selection must be assumed to act, if it acts at all, has been determined, and the selective death-rate has been exhibited as a function of the abnormality, while a numerical estimate which is at least of the same order as the amount of the selective destruction has been obtained.

The method by which the result described has been arrived at is likely to be capable of application to a very considerable number of cases. Mathematically considered, the conditions which were at first assumed and then proved to obtain in the organ discussed are by no means general. It is necessary for the employment of this method that the variations should be distributed on each side of the mean with sensible symmetry, and that the position of minimum selective destruction should be sensibly coincident with the mean of the whole system. Such statistical information as is at present available leads to the belief that these conditions may be expected to hold for a large number of species, which are sensibly in equilibrium with their present surroundings, so that their mean character is sensibly the best, and the change of mean from generation to generation is at least very small. They cannot be expected to hold in cases of rapid change such as those induced artificially by selection under domestication, or naturally by rapid migration or other phenomena resulting in a rapid change of environment.

For the investigation of such rapid change, it would be necessary to treat the more general case, in which the chances of deviations of opposite sign are not sensibly symmetrical, and in which the mean is not necessarily the position of minimum destruction. The treatment of this case requires the help of a professional mathematician.

It will be well to mention here a curious indirect confirmation of the result just described, based on evidence derived from a quite different source.

An attempt has been made to show that physiological accidents of a kind leading to change in the length of a portion of the carapace affect a crab at a rate measured by the value of the quantity $1-e^{-kx^2}$. The symmetrical distribution of variations from the mean which has been shown, especially by Mr. Galton, to occur in dimensions of weight, length, muscular strength, and other characters of various

organs in men, moths, sweet peas, and other things at various periods of life, made it seem probable that if selective destruction could be shown to occur in these cases, the expression for intensity of destruction, in terms of the deviation, would in all these cases be of the same form as that already arrived at. That is to say, the expression for the effect of physiological accidents of a number of different kinds, affecting a number of organisms in no way specially alike, is probably always of the same form. The question at once occurred, whether this expression might not be of general application, as a measure of the effect of physiological accidents upon the animal body.

The most convenient case in which to look for an answer to this question is the case of muscular tissue, in which the effect of accidents of stimulus can be readily measured. The recent paper of Cyboulski and Zanietowski (Pflüger's 'Archiv f. Physiologie,' Bd. 56, p. 45) gives an excellent series of data for determining the relation between energy of stimulus applied to a nerve, and effect upon the muscle, as measured by energy of external work performed in contraction. These observers give a large series of tables, in which the energy of stimulus, applied by discharging a condenser of known electrical capacity through a nerve, is given in one column, and in another is the work done by the muscle stimulated, measured by the height through which a known mass is lifted.

As is well known, the application of stimuli of less than a certain magnitude produces no muscular contraction; but if the maximum stimulus which can be applied without causing a contraction be reckoned as zero, the subsequent relation between stimulus and contraction does, in fact, agree very closely with that indicated by successive values of the quantity $1-e^{-kx^2}$.

In spite of the evident care and skill with which Cyboulski and Zanietowski have performed their experiments, their curves are slightly irregular. In order to minimise the effect of these slight irregularities, three of their results were treated in the following way:—In each system of observations the maximum subliminal stimulus was subtracted from the magnitude of the applied stimulus in each case; the three numbers representing the height of the muscle contraction for unit stimulus beyond this point in the three cases were added together; and so on throughout. The result is plotted in fig. 5, the height of the sum of three contractions being indicated by the ordinates of the points ⊙; the intensity of the corresponding stimulus, *minus* the subliminal stimulus, being measured along the abscissa.

The dotted curve is given by

$$g = 1-e^{-x^2/(8\cdot 15)^2},$$

was found that if the ova were placed in water at about 8° or 25° C. for an hour, or even for a minute, at the time of impregnation, the resulting larvæ after eight days development were, on an average, 4·6 per cent. smaller than those impregnated at from 17° to 22°, though all the subsequent conditions of development were identical. If kept at the abnormal temperature for only ten seconds during impregnation, the resulting larvæ were only 1·7 per cent. smaller, probably because the time was too short for all the ova to become impregnated under the abnormal conditions. Differences of temperature during the course of development have in comparison a much slighter influence. Thus larvæ allowed to develop in water at 17° to 22° are only about 2 per cent. larger than those allowed to develop at 15·7° to 17°, or at 22° to 23·7°.

The time of the year when the artificial fertilisations are prepared has a very marked influence on the size of the larvæ. Thus, those obtained in the middle of August are about 20 per cent. smaller than those obtained in April, May, and October, whilst those obtained in June and July are intermediate in size. This is probably due to the comparative immaturity of the ova and spermatozoa in the off-breeding season.

The salinity of the water has a great influence on the development. Larvæ allowed to develop in water containing 50 c.c. of distilled water per litre are 15·6 per cent. larger than those grown under normal conditions, and those in water containing 25 c.c. per litre 9·5 per cent. larger. On the other hand, in water containing 150 c.c. of distilled water per litre they are 4·3 per cent. smaller. In water more concentrated than the normal they remain practically unchanged, but larvæ grown under normal conditions from ova impregnated in concentrated water are 1·6 per cent. larger.

It was thought to be of interest to determine the effects which the various colours of the spectrum have upon development, though these are not conditions of environment which occur in nature. Larvæ allowed to develop in the blue light of copper sulphate solution are 4·5 per cent. smaller than the normal, and those in the violet-blue light of Lyons blue solution 7·4 per cent. smaller. Those grown in the green light of nickel nitrate solution are 4·8 per cent. smaller, and those in red and yellow lights respectively 6·9 per cent. and 8·9 per cent. smaller. The development of the larvæ seems to be but little affected if it is carried out in absolute darkness, the size only being diminished by 1·3 per cent. Larvæ grown in semi-darkness are apparently 2·5 per cent. larger than the normal.

The body-length of the larvæ appears to be uninfluenced by the number of larvæ developing together in a given volume of water, if it be kept below 30,000 per litre. The arm-lengths are, on the other hand, considerably affected. The aboral and oral arm-lengths of larvæ grown in water containing 4,000 per litre are respectively 13·4 and 15·9 per cent. shorter than of those in water containing 500 per litre; in water containing 17,500 per litre they are 25·9 and 23·3 per cent. shorter, and in water containing over 30,000 per litre 53·0 and 43·2 per cent. shorter.

Certain products of metabolism exert a favourable influence on the developments of the larvæ, and not, as would be naturally expected, a harmful one. Thus larvæ allowed to develop in water containing 1 in 70,400 of uric acid are 12·2 per cent. larger than those grown in ordinary water. It is only when the proportion of uric acid is increased to 1 in 28,000 that an unfavourable influence is exerted, the larvæ being in this case 2·1 per cent. smaller than the normal. In water containing about 1 in 60,000 of urea the larvæ are about 3 per cent. larger. Again, the amount of carbonic acid gas dissolved in the water may be increased by some 18 per cent., so that it is only just insufficient to kill off the larvæ entirely, and yet no diminution in the growth is observed. On the other hand, some of the products of metabolism may produce a considerable decrease in the size of the larvæ, for larvæ grown in water in which other larvæ had previously developed were 7·6 per cent. smaller than the normal.

Thus far the effects of environment on the body-length only of the larvæ have been mentioned. As the arm-lengths were also measured, the effects upon them can also be determined. As a rule, the tissues of the arms seemed to be much more sensitive to environmental conditions than the tissues of the body, and considerable changes were frequently found to occur in them without any obvious cause. The conclusions to be drawn from these measurements are therefore not so unequivocal as from those of the body-length. The most important point established is that one and the same change of environmental condition may frequently produce opposite effects on the arm and body-lengths. Thus the body-lengths of larvæ allowed to develop at temperatures above 22° are about 2 per cent. smaller than of those at about 20°, but the aboral and oral arms are respectively 10·8 per cent. and 8·5 per cent. *longer*. Again, the body-length of larvæ developed in diluted water is on an average increased by 9·1 per cent., but the arm-lengths are *decreased* by 7·7 per cent. and 10·5 per cent., or, as the arm-lengths are percentages on the body-lengths, they are practically not affected at all. Also the arm-lengths are not affected to the same extent by the same change of environmental condition. Thus the ratio between the arm-lengths is 4·3 per cent. higher at temperatures above 22° than at those below 18°.

As the number of measurements made was so large, it was thought to be of interest to subject them to statistical examination. It was found that with the body-length and oral arm-length measurements the deviations from the average occurred with a frequency indicate by the theoretical law of error. The measurements of the abor arm-length did not agree so well, possibly owing to dimorphis The relative probable error of the smaller larvæ was greater tha that of the larger ones, in the proportion of 63 to 57. As most the smaller larvæ were obtained in the summer months, with pr sumably not quite mature ova, it is probable that the variability i the size, as well as the actual size of the larvæ, is affected by th time of the year in which the fertilisations are made. The varia bility is also affected by the temperature of development, it reachin a maximum at 18° to 20°. The variability of the organs of echin derm larvæ is much greater than in the case of higher animal Thus the probable error of the body-length is 6·1 per cent., of th aboral arm-length 11·3 per cent., and of the oral arm-length 9·4 pe cent.

Presents, February 28, 1895.

Transactions.

Acireale:—Accademia di Scienze, Lettere e Arti dei Zelanti. Att e Rendiconti. Vol. V. 8vo. *Acireale* 1894. The Academy

Buda-Pesth:—Ungarische Geologische Gesellschaft. Földtan Közlöny. Kötet XXIV. Füzet 11—12. 8vo. *Budapest* 1894 Jahresbericht. 1892. 8vo. *Budapest* 1894. The Society

Cracow:—Académie des Sciences. Bulletin International. Janvier 1895. 8vo. *Cracovie.* The Academy

Frankfort-on-the-Main:—Senckenbergische Naturforschende Ge sellschaft. Abhandlungen. Bd. XVIII. Heft 4. 8vo. *Frank furt a. M.* 1895. The Society

Leipsic:—Astronomische Gesellschaft. Vierteljahrsschrift. Jahrg XXIX. Heft 4. 8vo. *Leipzig* 1894. The Society

Königl. Sächsische Gesellschaft der Wissenschaften. Abhand lungen (Math.-phys. Classe). Bd. XXI. No. 3. 8vo *Leipzig* 1894. The Society

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Observations and Reports.

Berlin:—Königliche Sternwarte. Astronomisches Jahrbuch. 1897 8vo. *Berlin* 1895. The Observatory

Madras:—Government Observatory. Results of Observations o the Fixed Stars made with the Meridian Circle in the Year 1883–87. 4to. *Madras* 1894. The Observatory

Milan:—R. Osservatorio Astronomico di Brera. Osservazion Meteorologiche. 1894. 4to. *Milano.* The Observatory

Observations and Reports (*continued*).

Paris:—Bureau des Longitudes. Éphémérides des Étoiles de Culmination Lunaire et de Longitude pour 1896. 4to. *Paris* 1894. The Bureau.

Washington:—Weather Bureau. Monthly Weather Review. October, 1894. 4to. *Washington.* The Bureau.

March 7, 1895.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates for election into the Society were read, as follows:—

Allen, Alfred Henry, F.C.S.
Barrett, Professor W. F.
Barry, J. Wolfe, M.Inst.C.E.
Bateman, Sir Frederic, M.D.
Bell, Robert.
Binnie, Alexander Richardson, M.Inst.C.E.
Blake, Rev. John Frederick, M.A.
Bourne, Professor Alfred Gibbs, D.Sc.
Bovey, Henry Taylor, M.A.
Bryan, George Hartley, M.A.
Burdett, Henry Charles.
Callaway, Charles, D.Sc.
Cardew, Philip, Major, R.E.
Clowes, Professor Frank, D.Sc.
Collie, J. Norman, Ph.D.
Corfield, William Henry, M.D.
Downing, Arthur Matthew Weld, M.A.
Elgar, Francis, LL.D.
Eliot, John, M.A.
Elwes, Henry John, F.L.S.
Etheridge, Robert, F.G.S.
Gray, Andrew, M.A.
Green, Professor Joseph Reynolds, D.Sc.
Griffiths, Ernest Howard, M.A.
Hamilton, Professor David James, M.D.
Harcourt, Leveson F. Vernon, M.Inst.C.E.
Haswell, Professor William A., D.Sc.
Head, Henry, M.D.
Heycock, Charles Thomas, M.A.
Hickson, Sydney John, M.A.
Hill, George Henry, M.Inst.C.E.
Hinde, George Jennings, Ph.D.
Holden, Henry C. L., Major, R.A.
Howes, Professor George Bond, F.L.S.
Kipping, F. Stanley, D.Sc.
Lansdell, Rev. Henry, D.D.
Lockwood, Charles Barrett, F.R.C.S.

McClean, Frank, M.A.
McConnell, James Frederick Parry, Surgeon-Major, F.R.C.P.
Macewen, Professor William, M.D.
McMahon, Charles Alexander, Lieut.-Gen.
Mansergh, James, M.Inst.C.E.
Martin, John Biddulph, M.A.
Martin, Sidney, M.D.
Matthey, Edward, F.C.S.
Miers, Henry Alexander, M.A.
Minchin, Professor George M., M.A.
Mott, Frederick Walker, M.D.
Murray, George Robert Milne.
Notter, James Lane, Surgeon-Lieut.-Col.
Oliver, John Ryder, Major-Gen., R.A.
Pearson, Professor Karl, M.A.
Power, William Henry.
Preston, Professor Thomas, M.A.
Purdie, Thomas, B.Sc.
Reade, Thomas Mellard, F.G.S.
Roberts, Ralph A., M.A.
Rutley, Frank, F.G.S.
Salomons, Sir David, M.A.
Scott, Alexander, M.A.
Stebbing, Rev. Thomas Roscoe Rede, M.A.
Stewart, Professor Charles, M.R.C.S.
Stirling, William, M.D.
Stuart, Professor T. P. Anderson, M.D.
Sutton, J. Bland.
Tanner, Professor Henry William Lloyd, M.A.
Thomson, Professor John Millar, F.C.S.
Trouton, Professor Frederick Thomas, M.A.
Truman, Edwin Thomas, M.R.C.S.
Turner, Professor Herbert Hall, M.A.
Waterhouse, James, Colonel.
Whymper, Edward, F.R.G.S.
Wilson, William E.
Wimshurst, James.
Woodhead, German Sims, M.D.
Woodward, Horace Bolingbroke, F.G.S.
Wynne, William Palmer, D.Sc.

The following Papers were read:—

I. "The Rubies of Burma and Associated Minerals—their Mode of Occurrence, Origin and Metamorphoses. A Contribution to the History of Corundum." By C. BARRINGTON BROWN, Esq., F.G.S., and J. W. JUDD, F.R.S., F.G.S., Professor of Geology, Royal College of Science. Received February 6, 1895.

(Abstract.)

This memoir embodies the results of a series of investigations which were initiated by the Right Hon. Viscount Cross, sometime H.M. Secretary of State for India, shortly after the annexation of Burma by the British Government. The researches were undertaken with a view to the determination of the value of the celebrated ruby mines of that country, and of the conditions under which the gem is found. One of the authors, Mr. C. Barrington Brown, visited

Upper Burma in 1887, and, under the auspices of the Governor-General of India—the Marquis of Ava—and with the assistance of the military and civil authorities of Burma, was enabled to make a geological survey of the district and prepare a report for the use of the Home Government. The large series of specimens collected to illustrate this report was entrusted to the other author for description, and he has prepared the portions of this memoir dealing with the petrology and mineralogy of the district.

It was in a memoir read before this Society in 1798 that the crystallised oxide of aluminium was shown by Greville to be a definite mineral species, to which he gave the name of "corundum;" while, in an appendix to this memoir, the Count de Bournon exactly determined the crystalline form of the mineral. Four years later, the last-mentioned author submitted a second paper to this Society, in which the mode of occurrence of the mineral in Ceylon and in the Salem District in Southern India was fully discussed. Twenty years later, Leschenault de la Tour, while on a scientific mission to Southern India, collected and sent to Paris a remarkable series of rocks from the gem-bearing districts. Quite recently, an able French mineralogist and petrographer, M. A. Lacroix, has described the series of specimens in the collections made by de Bournon and Leschenault de la Tour. Much light has been thrown on the mode of occurrence of the corundum in India by the labours of Mr. F. M. Mallet, Dr. V. Ball, and other members of the Geological Survey of that country; the remarkable emery deposits of Asia Minor have been thoroughly studied by the late J. Lawrence Smith and Professor G. Tschermak; while the occurrence of corundum in the Eastern States of North America has formed the subject of important memoirs by Dr. Genth and other authors.

The famous ruby district of Upper Burma was almost unknown to Europeans before the annexation of the country by the British. It is situated about 90 miles N.N.E. of Mandalay, and about 11 miles E. of the military post of Thebayetkin, on the Irrawaddy. The tract, so far as explored, is about 26 miles long and 12 broad, and lies at elevations varying from 4,000 to 5,500 feet above the sea-level. The principal mining centre in this district is Mogok, and the present workings for rubies extend over an area of 45 square miles; old workings, however, being found over an area of 66 square miles. The principal mining operations are carried on in the three valleys of Mogok, Kathay, and Kyatpyen; but there are some smaller outlying districts, in which mines were formerly worked, in the Injouk Valley, near Bernardmyo, at Wapudoung, 11 miles E. of Thebayetkin, and at Launzee, 8 miles S.W. of Kyatpyen. There is also a small tract of ruby-bearing rocks (crystalline limestones) at Sagyin, 24 miles N. of Mandalay; and it is asserted by the natives that two other limestone hills, 15 miles N. of Sagyin, have yielded rubies; while old ruby workings were found in making the railway at Kauksay, 30 miles S. of Mandalay. It is also probable that ruby-bearing limestones, and the alluvial earths derived from them, may be found in portions of the adjoining Shan States. Indeed, at a point about 25 miles southward from Mogok, in the Shan State of Mainglôn, Dr. F. Noetling, of the Geological Survey of India, has found that rubies have been obtained from the alluvium of a stream that flows from the mountains that lie considerably to the S.E. of the Mogok District.

The rubellite (red tourmaline) of the same district was found by Mr. Barrington Brown not to occur in association with the rubies, but to come from certain gneisses and schistose rocks. The locality which yields this gem, so highly prized by the Chinese, is Nyoungouk, 10 miles S.E. of Mogok; the alluvium which yields the rubellite appears never to contain rubies and spinels. Black tourmaline (schorl) has been extensively worked, as shown by Dr. F. Noetling, in the Shan State of Mainglôn, not far from the rubellite locality.

In the mountainous tract which includes the ruby districts, the general trend of the hill ranges is from east to west. The bottom of the Mogok Valley, in which the principal workings are situated, lies at a height of 4,100 ft. above the sea; while the loftiest mountains of the range to the north and east are the Chenedoung Peak, 7,362 ft., and the Taungnee Peak 7,775 ft. above the sea-level. The alluvia of the valleys of Mogok, Kathay and Kyatpyen are formed by streams flowing southwards from this mountain chain; while those of the valleys of Injouk and Kabein are deposited by streams flowing in the opposite direction. The district, which is a somewhat malarious one, has an annual rainfall of about 80 inches; bût in March, April, and May, the supply of water for mining operations is deficient.

The mountains are composed of various gneissic and granulitic rocks, occasionally passing into schists. Subordinate to the general mass of gneisses, often containing garnets, are certain peculiar varieties of foliated and massive rocks, including both acid and basic types, with limestone bands, often of a highly crystalline character. It was in these limestones that the rubies and spinels were found to be embedded, associated with graphite, phlogopite, pyrrhotite, and many other minerals. The sides of the hills are found to be shrouded in a deposit of hill-wash, often 50 ft. in thickness, composed of fragments, derived from the mountains, embedded in a clayey matrix. On the bottoms of the larger valleys there are extensive level deposits of alluvial matter, consisting of brown, sandy clay, resting on coarse gravels, which in turn cover other argillaceous beds. It is in these lower clay beds of the river alluvia, and in similar deposits formed in gullies in the hill-wash, that the rubies, spinels, and other gems of the district are found.

Mining operations for the obtaining of rubies are carried on in Burma in four different ways. (1.) In the alluvia, "twinlones," square pits from 2 to 9 ft. across, ingeniously timbered with bamboo, are sunk to the ruby earth, the drainage of the pits and the removal of material being effected by baskets attached to balance poles, both made of bamboo. (2.) In the hill-wash long open trenches, called "hmyaudwins," are carried from the sides of a gully, and the earth is washed out by streams conveyed into the trenches by bamboo pipes. (3.) In the caves and fissures filled with earth which abound in the limestone rocks, regular mines—"loodwins"—are opened, and the productive ruby earth is followed for long distances by means of shafts and galleries. (4.) The limestones which contain the rubies are at one or two points quarried, and the gems are obtained by breaking up the rock masses.

The extensive rubellite mines at Nyoungouk are worked in a somewhat similar plan to the "hmyaudwins." Water is delivered by a number of bamboo pipes at the head of the almost vertically exposed faces of alluvium; and as the masses of the latter are loosened, the miners dash water upon them from shovel-shaped baskets, and are able to detect and pick out by hand the brilliantly coloured stones exposed on the washed surfaces.

The petrology of this district of Upper Burma, in which the rubies, spinels, and rubellite occur, presents features of the greatest geological interest. In many respects the petrology of Burma exhibits close analogies with that of the corundiferous localities of Ceylon, the Salem District, and other portions of the Indian peninsula; but some of the phenomena presented by the rocks of the Burma ruby district do not appear to find a parallel in any of the gem-yielding tracts described by de Bournon and more recently by Lacroix.

The general mass of gneissic rocks composing the mountainous district in which the ruby localities are situated are of intermediate chemical composition, and consist of biotite-gneisses, biotite-granulites, and, more rarely, biotite schists—rocks in which hornblende is rare or altogether absent, but which, on the other hand, are often remarkably rich in garnets. Neither corundum nor spinel have been certainly detected in these rocks.

Interfoliated with these ordinary gneissic rocks, which form the great mass of the mountains, we find rocks of much more acid composition, including very coarse pegmatites and graphic granites, aplites and granulites (leptynite or Weiss-stein), granular quartzites, and orthoclase-epidote rocks. The orthoclase of these rocks frequently contains inclusions of fibrolite and other minerals, it often exhibits the "murchisonite" modification and partings, and is not unfrequently converted into "moonstone;" still more complete alterations of the orthoclase into epidote, muscovite, and kaolin being by no means uncommon. In the rubellite district of Nyoungouk these acid rocks contain pink and blue tourmaline (rubellite and indicolite), often beautifully zoned, and it is probably from rocks of this class that the fine gem rubellites are derived.

Of still greater interest are certain other subordinate rocks of basic and sometimes ultra-basic composition. These include the remarkable pyroxene-gneisses and pyroxene-granulites, which have in recent years been described as occurring in so many widely-scattered regions —such as Ceylon, Southern India, Central and Southern Europe, Norway and Sweden, Brittany, Spain, Algeria, Eastern, Western, and Southern Africa, the United States and Canada, Brazil, and New Caledonia. In these rocks the felspars are for the most part basic ones, near to anorthite; the crystals almost always exhibit the phenomenon described by French petrographers as "quartz of corrosion," and the partial or complete transformation of these felspars into scapolite ("werneritisation") can frequently be traced. The ferro-magnesian silicates are represented by many varieties of augite (sahlite, diopside, and ægerine), of enstatite (bronzite and hypersthene), and more rarely of hornblende. Garnets are a frequent and abundant constituent in many of these rocks, which, in their accessory minerals and their structures often exhibit many features of striking interest. By the gradual disappearance of the felspars from these rocks, they pass into remarkable varieties of pyroxenites and amphibolites. The chief varieties of these rocks, which are now described from Burma, are the following:—Augite-gneiss (with sahlite, green diopside, &c.), augite-granulites (very rich in garnet), enstatite-gneiss (with bronzite or hypersthene), enstatite-granulites (rich in garnet), scapolite-gneisses, scapolite-granulites, pyroxenites and amphibolites of many varieties, and lapis-lazuli (lazurite-diopside-epidote rock). Many of these rocks contain crystals of calcite scattered through them.

It is with these basic rocks, and more especially with the ultrabasic types last mentioned, that the remarkable crystalline limestones that contain the rubies and spinels are most intimately associated; indeed the passage of rocks consisting of various silicates with a few calcite crystals into masses principally composed of calcite, but with the silicate minerals and oxides dispersed through them, is of the most insensible kind. Some of the ruby-bearing limestones are highly micaceous ("cipollinos"), others are "calciphyres," in some of which the individual calcite crystals attain enormous dimensions. With the rubies and spinels are found a great number of oxides and silicates, both original and secondary, with much graphite and pyrrhotite.

In the gravels and clays of the district fine specimens of the minerals derived from the atmospheric degradation of the limestones and other rocks are found, sometimes broken and waterworn, at other times almost uninjured.

The study of the extensive series of minerals brought from the ruby mines of Burma is calculated to throw light upon many important scientific problems.

The association of minerals in the remarkable crystalline limestones of Burma is worthy of the most careful consideration. Corundum—in its various forms of ruby, sapphire, white sapphire, oriental amethyst, oriental topaz, &c.—is found associated with red, purple, brown, black and other spinels, the relative proportions of the minerals composed of aluminium oxide and of magnesium aluminate being very variable. The other minerals present in the crystalline limestones are zircon (rare); garnets (abundant in some places); a remarkable blue apatite; felspars, of many species and varieties (including murchisonite, moonstone, sunstone, &c.), and in every stage of alteration; quartz (in many varieties, and exhibiting some remarkable peculiarities of crystallisation); micas (phlogopite, fuchsite, with muscovite and other secondary and so-called hydromicas); hornblende and arfvedsonite; augite (sahlite, diopside, and ægyrine); enstatite (bronzite and hypersthene); wollastonite; lapis-lazuli; fibrolite; scapolite; with graphite and pyrrhotite. In addition to muscovite and other secondary micas, we find the following alteration products:—Diaspore, margarite, and other clintonites, chlorites, vermiculites, and carbonates.

It is a noteworthy circumstance that none of the silicates combined with fluorine and boron compounds—such as topaz, tourmaline, chondrodite and humite, axinite, or datholite have been certainly detected in these limestones. Beryl (aquamarine) and danburite have been said to occur in the ruby earths, but there is reason for doubting the correctness of the statement. The limestone which, in the association of minerals found in it, most closely resembles the rock of Burma, is the remarkable white limestone of Orange County, N. Y., and Sussex County, N. J.; but in the American rock the corundum and spinels are associated with tourmalines and chondrodites.

In considering the question of the *origin* of the corundums and spinels of Burma, there are several very important facts to be borne in mind. The gems, when found *in situ*, always appear to occur in the limestone, and this limestone is of a very remarkable character. There are no facts which point to the conclusion that the limestone was originally of organic origin, but many circumstances suggest that it may have been formed by purely chemical processes going on at great depths within the earth's crust. The highly-crystalline calcareous rock, besides containing so many silicates and oxides, is associated in the most intimate manner with pyroxene-gneisses and granulites containing anorthite, and with various pyroxenites and amphibolites. The lime felspars and lime-soda felspars of these rocks show the greatest tendency to undergo change—passing into scapolites by the process known as "werneritisation," and eventually giving rise to the separation of calcium carbonate and hydrated aluminium silicates. That from the last-mentioned salts the hydrated oxides of aluminium (diaspore, gibbsite, bauxite, &c.) may be separated has been shown by the studies of Liebrich and others, while the conversion of these substances into the anhydrous aluminium oxide has been shown to take place by H. St. Claire Deville, Stanislas Meunier, and others.

Crystallised aluminium oxide (corundum) has now been formed by chemists by no less than 20 different processes, and in some cases, like those described by Senarmont, Weinschenk, Bruhns, and Friedel, the formation and crystallisation of the substance has been effected at very moderate temperatures under pressure. By one or other of these or similar methods, it is probably that the formation of the Burma corundum and spinel has been effected, the source of the minerals being the decomposition products of basic and easily-altered lime felspars in the pyroxene-gneisses.

Of still greater interest than the question of the origin of the corundums and spinels are the problems connected with the remarkable changes that these minerals undergo in deep-seated rock masses. The rubies of Burma, when found *in situ* in the limestones, are usually seen to be enveloped in a mass of materials produced by the alteration of their superficial portions. Nearest to the unaltered gem is a zone of diaspore—the hydrated aluminium oxide—and this is found to pass insensibly into various hydrous aluminous silicates—margarites and other clintonites, vermiculites, muscovites, kaolinites, &c. While, in some instances, the corrosion of the rubies appears to have gone on in a seemingly irregular manner, in the majority of cases a very definite mode of metamorphosis may be detected by the study of the various examples. There are evidently certain planes of "chemical weakness" (analogous to the cleavage planes, gliding planes, and other directions of physical weakness) along which decomposition goes on most readily. The principal of these solution planes is the basal plane, and parallel to it we find the gems eaten away in a series of step-like surfaces. Other less pronounced planes of chemical weakness exist parallel to the prism faces. Unaltered corundum is, like quartz, destitute of true cleavage, and breaks with a perfectly conchoidal fracture. If, however, gliding planes and lamellar twinning be developed in corundum (like those so easily produced in the same way in calcite), parallel to the fundamental rhombohedron of the crystals, then these gliding planes become "solution planes," along which chemical action takes place most readily. Along the primary or secondary solution planes, hydration of the aluminium oxide takes place, and diaspore is formed, as shown by Lawrence Smith and Genth, and this unstable mineral enters into combination with silica and other oxides present to give rise to the numerous pseudomorphs of corundum, which are so well known to mineralogists.

There are certain crystals of corundum and spinel from Burma, which present illustrations of corrosion of a very remarkable and interesting character. Commencing with the formation of naturally etched figures ("Verwitterungsfiguren") the work of corrosion goes on till the whole crystal is broken up into an aggregate of simple forms—these being, in the case of the spinel, the octahedron, and in the case of the corundum, a combination of the rhombohedron, basal plane, and prism.

It is interesting to note that the quartz, felspars, and other minerals associated with the rubies and spinels of Burma, exhibit phenomena of external etching and internal chemical change similar to those we have been describing in the case of the gems. The study of the whole of the phenomena throws much new light on the remarkable changes which take place, at great depth in the earth's crust, in minerals which, at the surface, appear to be of a very stable character.

II. "The Action of Heat upon Ethylene. II." By VIVIAN B. LEWES, Professor of Chemistry at the Royal Naval College, Greenwich. Communicated by Professor T. E. THORPE, F.R.S. Received January 10, 1895.

In a paper communicated to this Society in the spring of 1894,* I showed that ethylene, when subjected to heat, was converted into acetylene and methane, according to the equation

$$3C_2H_4 = 2C_2H_2+2CH_4,$$

and that the acetylene so formed either at once polymerised, forming a large number of secondary products, or else decomposed to carbon and hydrogen, according to the temperature at which the action was being carried on.

The fact that ethylene is one of the principal products in many cases of destructive distillation, renders a knowledge of the conditions affecting these changes of considerable importance, and the experiments described in this paper were made with the view of ascertaining the effect of rate of flow, area of heated surface, and dilution upon the changes taking place.

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* 'Roy. Soc. Proc.,' vol. 55, p. 90.

The apparatus and methods of analysis employed were the same as described in the former paper, with the exception that a platinum tube, 14 mm. in diameter and 73·6 cm. long, was used as the decomposing chamber, and that an easily regulated gas combustion furnace was employed as the source of heat. The temperature of the gas in the tube was measured as before, by the Le Chatelier thermo-couple, and every precaution was taken to keep it constant during the experiments.

The ethylene in each case was prepared by heating a mixture of 25 parts, by weight, of alcohol and 150 of strong sulphuric acid, purifying, and storing in a large glass holder.

The first set of experiments was to ascertain the influence of area of heated surface upon ethylene when passing through a tube heated to the temperature most often employed in the destructive distillation of coal and hydrocarbon oils, and also to find the result of varying the rate at which the gas was passed through the zone of heat.

Table I.—Influence of Rate of Flow and Length of Heated Surface upon the Amount of Ethylene decomposed. Temperature, 900° C. Rate of Flow, 5 c.c. per minute.

Length of tube heated	1 inch.	6 inches.	18 inches.
Volume of gas—before heating ..	525	625	625
„ „ after „ ..	490	653	670
Analysis of gaseous products.			
Carbon dioxide	0·75	0·50	0·75
Oxygen	0·50	0·25	1·00
Unsaturated hydrocarbons	17·80	10·75	2·75
Carbon monoxide	2·21	1·75	1·75
Saturated hydrocarbons by absorp.	11·25 } 46·87	9·50 } 60·75	12·22 } 65·42
„ „ by explos.	35·62	51·25	53·20
Hydrogen	24·12	22·75	24·00
Nitrogen	7·75	3·25	4·33

On now calculating the percentage for the change in volume, we obtain the following figures:—

Length of tube heated	1 inch.	6 inches.	18 inches.
Unsaturated hydrocarbons......	16·60	11·25	2·94
Saturated hydrocarbons	43·72	63·42	70·13
Hydrogen	22·50	23·75	25·72

The original gas, however, only contained 96·7 per cent. of ethylene, so that the percentages of unsaturated hydrocarbons decomposed are:—

Length of tube heated	1 inch.	6 inches.	18 inches.
	80·1	85·48	93·76

Table II.—Temperature, 900° C. Rate of Flow, 15 c.c.

Length of tube heated	1 inch.	6 inches.	18 inches.
Volume of gas—before heating..	683	600	600
„ „ after „ ..	615	515	640
Analysis of gaseous products.			
Carbon dioxide	0·20	1·00	0·50
Oxygen	0·50	0·50	0·75
Unsaturated hydrocarbons	28·75	20·15	6·00
Carbon monoxide	1·50	1·50	1·75
Saturated hydrocarbons by absorp.	8·80 } 42·25	9·43 } 51·81	19·22 } 56·72
„ „ by explos.	33·45	42·37	37·50
Hydrogen	21·15	20·55	28·75
Nitrogen	5·65	4·50	5·53

On now calculating the percentage for the change in volume, we obtain the following figures:—

Length of tube heated	1 inch.	6 inches.	18 inches.
Unsaturated hydrocarbons......	25·87	17·28	6·39
Saturated hydrocarbons	38·02	44·44	60·46
Hydrogen	19·03	17·63	30·64

The original gas contained 96·7 per cent. of ethylene, so that the percentages of unsaturated hydrocarbons decomposed are—

Length of tube heated	1 inch.	6 inches.	18 inches.
	70·83	79·42	90·31

Table III.—Temperature, 900° C. Rate of Flow, 40 c.c.

Length of tube heated	1 inch.	6 inches.	18 inches.
Volume of gas—before heating ..	870	1050	1000
„ „ after „ ..	790	985	980
Analysis of gaseous products.			
Carbon dioxide	0·75	0·50	0·25
Oxygen	0·50	0·75	0·50
Unsaturated hydrocarbons	50·80	35·75	9·75
Carbon monoxide	1·50	1·00	1·50
Saturated hydrocarbons by absorp.	11·22 } 27·67	11·25 } 41·75	10·00 } 56·50
„ „ by explos.	16·45	30·50	46·50
Hydrogen	14·50	16·65	28·00
Nitrogen	4·28	3·60	4·00

Calculating the percentage for the change in volume, we obtain the following figures:—

Length of tube heated..........	1 inch.	6 inches.	18 inches.
Unsaturated hydrocarbons......	46·12	33·53	9·55
Saturated hydrocarbons	25·12	39·16	55·37
Hydrogen	13·16	15·61	27·44

The original gas contained 96·7 per cent. of ethylene, so that the percentages of unsaturated hydrocarbons decomposed are—

Length of tube heated	1 inch.	6 inches.	18 inches.
	50·58	63·17	87·15

On collecting these results in one table—

Percentage of ethylene decomposed.			
Length of tube heated	1 inch.	6 inches.	18 inches.
5 c.c.	80·10	85·48	93·76
15 c.c.	70·83	79·42	90·31
40 c.c.	50·58	63·17	87·15

The first thing that strikes one is the enormous amount of decomposition which takes place in the first inch of flow through the heated tube, and the small effect which an increase in the length of the heated surface has upon the further decomposition of the unsaturated hydrocarbons.

This result might be caused by the methane and hydrogen formed during the decomposition diluting the remaining ethylene, and so rendering the decomposition more difficult; it may also arise from secondary actions taking place amongst the primary products, and again forming ethylene, or it may be due to both these causes acting together.

In order to trace the effect of dilution, a series of experiments was made in which ethylene, diluted with hydrogen, was passed through 6 inches of the same tube as was used in the previous experiments, heated to the same temperature, and under precisely similar conditions.

The store holder of ethylene used for making the mixture contained a gas which gave, on analysis,

Ethylene	97·85
Nitrogen	2·00
Oxygen	0·15

Table IV.—Influence of Dilution in Checking Decomposition of Ethylene. Length of tube heated, 6 inches. Rate of Flow, 10 c.c. per minute. Temperature, 900° C.

Percentage of hydrogen	nil		10·0		25	
„ ethylene	98·7		90·0		75	
Volume of gas—before heating	100·0		100·0		100	
„ „ after „	81·3		69·5		69	
Analysis of products of decomposition.						
Carbon dioxide	1·00		0·75		0·50	
Oxygen	0·50		0·25		0·25	
*Unsaturated hydrocarbons	32·52		31·00		29·00	
Carbon monoxide	1·00		1·25		1·00	
Saturated hydrocarbons by absorp.	17·75	50·25	14·50	50·75	14·50	48·45
„ „ by explos.	32·50		36·25		33·95	
Hydrogen	12·62		14·25		18·50	
Nitrogen	2·11		1·75		2·30	
*Containing acetylene	1·06		0·52		0·73	

Percentage of hydrogen	50		75		95	
„ ethylene	50		25		5	
Volume of gas—before heating ..	100		100		100	
„ „ after „ ..	64		85		100	
Analysis of products of decomposition.						
Carbon dioxide	1·25		0·50		—	
Oxygen	—		0·15		—	
*Unsaturated hydrocarbons	22·25		11·10		2·00	
Carbon monoxide	1·25		1·00		0·50	
Saturated hydrocarbons by absorp.	8·50	36·00	5·50	15·80	0·75	4·85
„ „ by explos.	27·50		10·30		4·10	
Hydrogen	37·15		68·45		89·15	
Nitrogen	2·10		3·00		3·50	
*Containing acetylene...........	0·32		0·15		trace	

On calculating these percentages for the change in volume, we obtain the following results:—

	I.	II.	III.	IV.	V.	VI.
Unsaturated hydrocarbons ..	25·58	21·18	19·50	14·04	9·30	2·00
Saturated hydrocarbons	40·85	35·27	33·43	23·04	13·43	4·85

The ethylene taken only contained 97·85 per cent. of the hydrocarbon, hence—

	I.	II.	III.	IV.	V.	VI.
Unsaturated hydrocarbons originally present	97·85	88·02	73·35	48·90	24·45	4·89
Unsaturated hydrocarbons present after heating	25·58	21·18	19·50	14·04	9·30	2·00
Unsaturated hydrocarbons decomposed	72·27	66·84	53·85	34·88	15·15	2·89

and calculating this to percentage of total ethylene decomposed—

I.	II.	III.	IV.	V.	VI.
73·86	75·94	73·24	71·30	61·96	59·10

which shows that dilution has practically no effect in retarding the decomposition until 75 per cent. of diluent is present, and also clearly points to its being radiant heat, and not contact with the heated sides of the tube, which is responsible for the largest proportion of the decomposition; since, had contact been the active factor, dilution, by reducing the number of impacts of the hydrocarbon molecules with the heated surface, would have shown a considerable decrease in decomposition.

Some information can be obtained as to the secondary reactions which accompany the main decomposition, by studying the proportions in which the products, other than ethylene, are present in the gases after heating.

Taking the experiments made upon the effect of length of heated tube and rate of flow, and tabulating the percentages of saturated hydrocarbons and hydrogen corrected for change of volume, we obtain the following results:—

Saturated Hydrocarbons.

Length of tube heated	1 inch.	6 inches.	18 inches.
5 c.c.	43·72	63·42	70·13
15 c.c.	38·02	44·44	60·46
40 c.c.	25·12	39·16	55·37

The temperature employed in these experiments is not sufficiently high to cause any large amount of the methane to decompose, so that the volume should, according to theory, approximate to two-thirds of the ethylene decomposed.

1 inch of heated tube.

Rate of flow per minute.	Ethylene decomposed.	Methane found.	Calculated.	Variation.
5 c.c.	80·10	43·72	53·40	−9·68
15 c.c.	70 83	38·02	47·22	−9·20
40 c.c.	50·58	25·12	33·72	−8·60
6 inches of heated tube.				
5 c.c.	85·48	63·42	56·98	+6·44
15 c.c.	79·42	44·44	52·94	−8·50
40 c.c.	63·17	39·16	42·12	+2·96
18 inches of heated tube.				
5 c.c.	93·76	70·13	64·50	+5·63
15 c.c.	90·31	60·46	60·20	+0·26
40 c.c.	87·15	55·37	58·10	−2·73

The fact that when only 1 in. of tube is heated, there is a fairly constant deficit of the kind to be expected at the temperature employed, and that when a greater length of heated tube is used with a 5 c.c. rate of flow, the deficit becomes a substantial surplus, at once suggests that methane is amongst the secondary as well as the primary products of decomposition.

In the dilution experiments, the larger contraction in the volume noticeable points to the diluting of the products favouring polymerisation.

Dilution.	Ethylene decomposed.	Methane.		Variation.
		Found.	Calculated.	
Nil..........	72·27	40·85	48·18	−7·33
10 per cent. ..	66·84	35·27	44·56	−9·29
25 " ..	53·85	33·43	35·90	−2·47
50 " ..	34·88	23·04	23·24	−0·20
75 " ..	15·15	13·43	10·10	+3·33
95 " ..	2·89	4·85	1·92	+2·93

It seems probable from the figures that when dilution reaches above 50 per cent., not only is decomposition of the methane retarded, but formation as a secondary product commences.

Ever since water gas has been in use it has been well known that it contained traces of methane and acetylene, under conditions which render it impossible for them to have been formed from hydrocarbons remaining in the incandescent fuel, and the probabilities are that they have been produced. the acetylene by direct combination of carbon and hydrogen, and the methane by its partial decomposition.

The formation of ethylene from nascent hydrogen and acetylene takes place at such temperatures as those employed, and the amount so formed and again broken up by the radiant heat is purely a function of mass; so that I conceive from these experiments, the ethylene at once to a great extent decomposes under the influence of sufficiently high radiant heat according to the equation:—

$$3C_2H_4 = 2C_2H_2 + 2CH_4,$$

and that the acetylene partly decomposes, the nascent hydrogen again uniting with more acetylene to reproduce ethylene, whilst other portions of the acetylene polymerise to benzene and other more complex hydrocarbons, and that, if the flow of this mixture be continued through a heated chamber, the action continues, the amount of ethylene regenerated becoming less and less, until it ceases to exist as a product of the decomposition.

It is well known that hydrogen will diffuse through ignited platinum into a vacuous space or even into other gases, and although no change in volume was observed when a mixture of 95 per cent. hydrogen and 5 per cent. ethylene was passed through the tube heated to 900° C., it seemed advisable to make special experiments to ascertain if any loss of hydrogen from this cause did take place at the temperature employed.

The apparatus having been fitted up as before, 6 in. of the tube were heated to between 900° and 1000° C., and a carefully measured volume of pure hydrogen was made to pass through it three times at a slow rate of flow, with the result that, after being brought back to its original temperature and pressure, it measured 99·5 per cent. of the volume taken, showing that error from this cause is not likely to have taken place.

A new series of experiments was now undertaken to ascertain, if possible, how increase of contact with the heated walls of the containing vessel affected the amount and character of the decomposition taking place.

In order to do this, a small platinum tube 2·5 mm. in diameter and 45·72 cm. long was taken in place of the one used in the previous experiments, which was 14 mm. in diameter. The area of the big tube therefore, as compared with the area of the small tube, was as 1·54 to 0·049, and in order to obtain the relative amount of decomposition it is manifest that the rate of flow must be the same in both tubes.

If the rate of flow in the big tube be 40 c.c. per minute, then

$$\frac{2{\cdot}5^2 \times 40}{14^2} = 1{\cdot}27$$

will give the required rate for the small tube.

Table V.—Temperature, 900° C. Rate of Flow, 1·27 c.c. per minute.

Length of tube heated	1 inch.	6 inches.	12 inches.
Volume of gas—before heating..	100	100	100
" " after " ..	80	91	97
Analysis of gaseous products.			
Carbon dioxide	1·00	0·75	0·75
Oxygen	0·25	0·00	0·25
*Unsaturated hydrocarbons	15·30	12·20	6·30
Carbon monoxide	0·75	1·00	1·00
Saturated hydrocarbons by absorp.	13·50 } 50·50	12·10 } 60·30	9·50 } 54·50
" " by explos.	37·00	48·20	45·00
Hydrogen	26·00	18·75	30·80
Nitrogen	6·20	7·00	6·40
*Containing acetylene	0·3	0·32	0·20

Correcting the unsaturated hydrocarbons for change in volume, we obtain undecomposed—

I.	II.	III.
12·00	10·81	5·92

The ethylene used contained 96·75 per cent. of unsaturated hydrocarbons, therefore the amount decomposed is—

I.	II.	III.
84·75	85·94	90·83

Turning now to the experiments made with the large tube and 40 c.c. a minute rate of flow, we find that the percentage of ethylene decomposed was—

I. 1 inch.	II. 6 inches.	III. 12 inches.
50·58	63·17	75·16 calctd.

an increase of decomposition with the small tube amounting to the following percentages :—

I.	II.	III.
33·93	22·48	15·48

showing that the maximum increase of 34 per cent. rapidly falls with increased length of flow, whilst the ratio of area of heated surface to the passing gas is 43·96/7·85, or 5·6 times as great in the small tube as in the large, yet only gives an increase of one-third at most in the decomposition.

From the results of these experiments it may be stated that—

1. The initial decomposition of ethylene by heat is very rapid, and requires but a short flow through a heated containing vessel, such primary decomposition, however, being but slowly completed, owing to secondary reactions, which tend to re-form ethylene.
2. Dilution has but little effect in retarding the decomposition of ethylene, unless it be very large.
3. Increase in rate of flow diminishes the amount of decomposition when the heated area is small, but rapidly diminishes in effect as the length of flow through a heated area increases.
4. The decomposition of ethylene is chiefly caused by radiant heat, the effect of which is very great as compared with the decomposition due to contact with heated surfaces.

I desire to acknowledge the valuable aid given me by Mr. F. B. Grundy in this investigation.

III. "On the Measurement of Pressures by the Crusher-Gauge." By W. KELLNER and W. H. DEERING. Communicated by Dr. W. ANDERSON, F.R.S. Received January 21, 1895.

The object of the experiments, the results of which are given in this paper, was to compare the indication of pressure (produced by the firing in a closed vessel of a gunpowder) by the crusher-gauge with a simultaneous estimation of the same pressure by another and simple method. This latter consisted in ascertaining the weight which was equal to the maximum pressure of the gases of the fired cordite (the smokeless gunpowder used in these experiments) acting on a valve closing gas-tight an opening in the explosion-vessel of area small enough to allow of the use of weights of manageable amount, not so small a fraction of the unit of surface (1 sq. in.) as to have to use a very large multiplier. Within the explosion-vessel, on the top of which were the valve and weights, was placed the crusher-gauge, the firing of the charge of cordite giving the two indications of pressure. In the "weights" method, a series of short steps was taken, weights in excess of that required being placed on the valve, the cordite fired, and it being observed whether the gases of explosion were blown out of, or kept in, the vessel. The amount of compression of the copper-crusher was then measured, and the corresponding pressure ascertained from the existing tables. In the next experiment the weights were lightened, and so on, until the gas blew out of the vessel, the mean of the pressure at the blow-out point and of that in the last experiment in which the gas was kept in the vessel being taken as the pressure required. A pressure by copper-crusher was taken in each experiment made, and the mean of the series taken; the compression of the crusher was the same in the experiments of a series, whether the gases blew out of, or were kept in, the explosion-vessel, the weights apparently not being raised before the compression of the copper had come to an end.

It may be not superfluous to mention that the copper-crusher is a small cylinder of copper, before compression 0·5 in. long and 0·326 in. in diameter, one end of which rests on the base of a steel case in which it is contained, the other end being in contact with one end of a movable piston of 0·461 in. diameter ($\frac{1}{6}$ sq. in. section), on the other end of which the gases exert their pressure; suitable provision is made to prevent entrance of gas to the gauge-case, and to hold the copper-crusher in position while allowing room for its widening in the middle by compression. The coppers are used previously pressed to about a ton short of the pressure to be measured.

Rumford, in 1797, used a "weights" method to ascertain the relation existing between the pressure produced by a fired gunpowder and the density of the charge $\left(\frac{\text{weight of gunpowder}}{\text{volume of explosion-vessel}}\right)$. He used charges of gunpowder from 1 to 18 grains in weight, density of charge varied from 0·04 to 0·76, and the pressures measured were consequently very high; the weight just lifted by the pressure of the powder gases was found by a series of trials.

Bunsen also ('Gasometrische Methoden') used the same method, of course very much altered in details, to ascertain the pressures produced by the combustion of some gases in oxygen; the gaseous mixtures were at 1 atmos. pressure, the increase of pressure produced on firing the mixtures was at highest about 10 atmos. (about $\frac{1}{15}$ ton per sq. in.).

In our experiments, the gases of the fired explosive served only as a medium for comparing the two methods of estimating pressure, so that the question of any lowering of temperature by heating of the explosion-vessel did not come into consideration.

This direct experimental method of checking the crusher-gauge indications is preferable to calculation of pressure from the ascertained amounts of permanent gases, water, and quantity of heat produced at the pressures in question, because the calculation requires knowledge of the specific heat of the gases of combustion at (in the case of cordite) about 3000° C., and although there is valuable information on the subject, it is scarcely sufficiently accurate for this application of it. Again, the compression of the copper-crusher measures the maximum pressure of the gases, when (at the temperature mentioned) some dissociation of carbonic acid would occur (or rather some oxygen and carbonic oxide would co-exist), while the quantity of heat is measured in the cooled-down gases when all oxygen and carbonic oxide have combined; here, also, it would not be practicable with existing information to make an accurate allowance for difference of heat-quantity under the conditions of the experiment. Further, in calculating the pressure of the gases from their calculated temperature, it is assumed that their coefficients of expansion which were determined for 0° to 100° C. are valid at 3000° C. (At 0°, air is only about 190° above, and water is 100° below, its boiling temperature.)

If temperature were independently known, the drawbacks just mentioned in the use of the coefficient of expansion, and of the volume of gases, still apply to the calculation of pressure; and, conversely, to the frequent case of calculating temperature from ascertained pressure.

The steel explosion-vessels used in our experiments were of about 120 c.c. capacity; they were closed by screw stoppers of about 4 ins. total length (the screw portion about 1⅛ ins. long), the end of which was screwed down to gas-tight contact with the circular seating forming the mouth of the explosion-chamber. The stoppers had a square head at starting 2 ins. long; the whole stopper was perforated axially with a cylindrical hole ¼ in. in diameter. The mouth of the hole in the stopper was closed by a steel ball of ½ in. in diameter (which formed the valve), a ball such as is used for ball-bearings; on the ball a 1-in. thick iron plate, about 15 ins. by 15 ins., was placed (the centre of the plate being perforated by a cylindrical hole ¼ in. in diameter, resting on the top of the ball), and on the plate the weights. The latter consisted of lead cylinders, weighing about 5 cwt. and 8 cwt. respectively (diameters 14 ins. and 12 ins.); of the former, at the most three (placed on top of each other), and of the latter, two, were used. The weight was made up with half-hundredweights and smaller weights placed on the top of the upper lead-cylinder; all the weights, lead-cylinders, iron plate, and iron rod, were weighed at the beginning and end of the experiments by standard weights.

The explosion-vessels had externally a collar and a hexagonal portion fitting into a hexagonal hole in an iron plate, which was screwed to a balk of timber, held in place by weights; this held the explosion-vessel while the screw-stopper was hammered down, and formed the base of the apparatus. The lead cylinders had an axial cylindrical hole to their centre; in the hole of the top cylinder an iron rod (about 1 in. diameter, 4 ft. long) was placed, the upper end of the rod passing loosely through a hole in a cross-beam kept in position by iron uprights on either side of the base of the apparatus. The weights were carefully balanced, so that pressure of iron rod against cross-beam was reduced to an inconsiderable amount. The lead cylinders were readily placed in position on the iron plate (held horizontal by wedges until cylinders and weights were in position) by means of a movable crane.

The firing of the cordite charge was effected by means of a wire-bridge heated to redness by an electric current; on the centre of the wire-bridge a small piece of the cordite was firmly hung. One end of the wire-bridge was soldered to the end of a steel pin forming part of the stopper, the other end was soldered to the end of a steel wire of $\frac{1}{30}$ in. diameter cemented by a suitable resinous cement in a hole of $\frac{1}{16}$ in. diameter passing through the length of the stopper. By reducing the area of the insulated wire to this small amount, the cement held the wire gas-tight against the highest pressure (16 tons per sq. in.) employed. In the 14-ton and 16-ton experiments, the stopper was cooled by a little ice placed on its upper surface, to prevent softening of the cement.

The area of the mouth of the hole in the stopper was measured by means of a microscope with a cross-wire, the microscope moving horizontally on a graduated scale and carrying a vernier. The pressure of the weights on the steel ball and stopper slightly bevels the mouth of the latter; at the commencement of a series of experiments, weights to about the maximum amount to be used were placed on the steel ball, and four diameters of the inner and lower circle of the bevelled contact-ring were then measured with the microscope-vernier. The area was re-measured in the course of several of the series, and found to be unchanged.

One hundred and twenty-six experiments in all were made; a few of these were lost by the weight required having been under-estimated and the gases blowing out on the first experiment of a series being made. A few of them also were lost by the blowing out of the insulated wire, or by other causes. The remainder form twenty-seven series (a series consisting of at least two experiments: in one of which the gases were kept in, and were blown out in the following one), the results (viz., the mean of the last two experiments) of which are given below. Eight series consisted of two experiments, two of three, six of four, three of five, six of six, one of seven, one of eight.

The gases were either wholly kept in, or wholly blown out of, the explosion vessel. In the former case there was no report and no damage to the steel ball, which could be used for all the experiments of a series; in the latter case there was a loud report, and the mouth of the hole in the stopper was slightly injured (the steel being melted and blown away by the gases), and a ring melted in the steel ball. A new steel ball was, of course, used for each series of experiments. For the experiments at the lower pressures of 5 and 6 tons, the hole in the stopper was (after damage by the blown-out gases) enlarged at the top (to a depth of $\frac{1}{8}$ in.) to 0·35 in., and subsequently to 0·40 in.

diameter, thus affording a check by varying area. For the 13- and 16-ton experiments, the 0·25-in. hole only was used, as the weights required for the larger holes would have been unmanageable with the means available at the time; in this case, when damage to the mouth of the hole in the stopper occurred by the blowing out of the gases, the head of the stopper was only planed down.

The amounts by which the weights were lightened in the last (and usually in each) experiment of a series was the following:—

	5 tons.	6 tons.	13 tons.	16 tons. 8 series.	
	1 series.	4 series.	2 series.	5 series.	3 series.
0·25-inch hole.	22 lbs.	14 lbs.	28 lbs.	28 lbs.	56 lbs.
	= 0·2 ton	= 0·13 ton	= 0·26 ton	= 0·26 ton	= 0·52 ton per sq. in.

	5 tons.	6 tons. 6 series.		
	1 series.	1 series.	1 series.	4 series.
0·35-inch hole.	56 lbs.	56 lbs.	28 lbs.	14 lbs.
	= 0·26 ton	= 0·27 ton	= 0·13 ton	= 0·06 ton per sq. in.

	5 tons.	6 tons.
	1 series.	4 series.
0·40-inch hole.	56 lbs.	28 lbs.
	= 0·2 ton	= 0·1 ton per sq. inch.

The weight-differences are, of course, calculated into tons per square inch on the *measured* areas of the holes, the diameters f which differed slightly from 0·25 in., &c. As mentioned above, half the weight-difference (the mean of the last two experiments of a series) is taken for the end result.

The following were the results obtained:—

Hole 0·25 inch diameter.

						Mean of the 4.	
By weights	5·7	7·0	7·0	6·9	6·8	6·9	Tons per sq. in.
By crusher	5·1	6·2	6·5	6·6	6·0	6·3	
Weights *minus* crusher	0·6	0·8	0·5	0·3	0·8	0·6	

Hole 0·35 inch diameter.

								Mean of the 6.	
By weights	5·9	7·0	7·0	6·8	6·9	7·1	7·2	7·0	Tons per sq. in.
By crusher	5·2	6·3	6·3	6·4	6·4	6·6	6·2	6·4	
Weights *minus* crusher	0·7	0·7	0·7	0·4	0·5	0·5	1·0	0·6	

Hole 0·40 inch diameter.

						Mean of the 4.	
By weights	5·9	6·8	7·0	7·3	7·1	7·0	Tons per sq. in.
By crusher	5·2	6·4	6·4	6·3	6·4	6·4	
Weights *minus* crusher	0·7	0·4	0·6	1·0	0·7	0·6	

Hole 0·25 inch diameter.

			Mean of the 2.									Mean of the 8.	
By weights	14·7	14·4	14·6	18·5	18·9	18·1	18·8	17·9	18·2	18·7	18·2	18·4	Tons per sq. in.
By crusher	13·2	13·0	13·1	15·7	15·9	15·6	15·8	15·9	15·9	15·8	15·6	15·8	
Weights *minus* crusher	1·5	1·4	1·5	2·8	3·0	2·5	3·0	2·0	2·3	2·9	2·6	2·6	

The pressure by weights was invariably higher than by crusher; at 5 tons (by crusher) the difference was about 11 per cent., at 6 tons about 9 per cent., at 13 tons about 11 per cent., at 16 tons about 16 per cent. At 5, 6, and 13 tons there is usually (but not always) less difference between the individual pressures by crusher given above than between the weights pressures; at 16 tons there is decidedly greater regularity in the crusher results than in those by weights; the crusher pressures are, however, the means of series, which favours them somewhat.

Presents, March 7, 1895.

Transactions.

London:—British Astronomical Association. Journal. Vol. V. No. 4. 8vo. *London* 1895. The Association.

Geologists' Association. Proceedings. Vol. XIV. Part 1. 8vo. *London* 1895. The Association.

Iron and Steel Institute. Journal. 1894. No. 2. 8vo. *London* 1895; Brief Index of the Papers contained in the Journal. 1869–94. 8vo. *London* 1895. The Institute.

Odontological Society. Transactions. Vol. XXVII. No. 3. 8vo. *London* 1895. The Society.

Royal College of Physicians. List of the Fellows, Members, Extra-Licentiates, and Licentiates. 1895. 8vo. *London.* The College.

Royal Meteorological Society. Quarterly Journal. Vol. XXI. No. 93. 8vo. *London* 1895; Meteorological Record. Vol. XIV. No. 54. 8vo. *London.* The Society.

Royal Microscopical Society. Journal. 1895. Part 1. 8vo. *London.* The Society.

Newcastle-on-Tyne:—North of England Institute of Mining and Mechanical Engineers. Transactions. Vol. XLIV. Part 2. 8vo. *Newcastle-on-Tyne* 1895. The Institute.

Penzance:—Royal Geological Society of Cornwall. Transactions. Vol. XI. Part 9. 8vo. *Penzance* 1895. The Society.

Santiago:—Sociedad Nacional de Mineria. Boletin. Año XI. No. 73. 4to. *Santiago de Chile* 1894. The Society.

Stockholm:—Kongl. Vetenskaps Akademie. Öfversigt. Årg. LI. No. 10. 8vo. *Stockholm* 1894. The Academy.

Toulouse:—Académie des Sciences, Inscriptions, et Belles-Lettres. Mémoires. Série 9. Tome VI. 8vo. *Toulouse* 1894. The Academy.

Faculté des Sciences. Annales. Tome IX. Fasc. 1. 4to. *Paris* 1895. The Faculty.

Burdett (H. C.) Burdett's Official Intelligence for 1895. 4to. *London.* The Author.

Fitzgerald (R. D.) Australian Orchids. Vol. II. Part V. Folio. *Sydney* 1894. Government Printing Office, Sydney.

Frederick the Great. Politische Correspondenz. Bd. XXI. 4to. *Berlin* 1894. K. Akademie der Wissenschaften, Berlin.

M'Kendrick (J. G.), F.R.S. To What End? Remarks on the Phenomena of Life. 8vo. *Glasgow* 1895. The Author.

Ormerod (E. A.) Report of Observations of Injurious Insects and Common Farm Pests during the Year 1894. 8vo. *London* 1895. The Authoress.

Suess (E.), For. Mem. R.S. Einige Bemerkungen über den Mond. 8vo. *Wien* 1895. The Author.

March 14, 1895.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Croonian Lecture was delivered as follows:—

CROONIAN LECTURE.—"On the Nature of Muscular Contraction." By TH. W. ENGELMANN, Professor of Physiology in the University of Utrecht. Received February 25, 1895.

I beg your kind attention for a few moments to some observations and considerations on the nature of muscular contraction. Besides its general biological interest, the subject may claim a particular interest for the members of this Society, for the venerable institution of these Croonian Lectures is especially devoted to the furtherance of the study of muscular life, and, moreover, the Royal Society has, through its Fellows, from Robert Hooke down to our friend Professor Schäfer, produced numerous researches on muscular structure and action which may be said to belong to the most valuable part of our scientific property.

It will not be my task to recall to your mind all those eminent Fellows of this Society who have helped us to get a clearer insight into this subject, yet I cannot refrain from reminding you of one of these men, who is honoured throughout the whole world as one of the founders of our present doctrine of muscular structure and action—a man whose name will live on, in my home in Holland, of all countries, associated with the memory of his great brotherly friend, Donders, and whom, to my great grief, I may see here no more—I mean Sir William Bowman.

But a few years ago I had the happiness of seeing him in Utrecht, and of observing how the interest which he took in a province of science in which he had gained his first laurels remained unchanged, even after the lapse of half a century. To behold him among this audience would have been my greatest joy and pride, and his judgment would, in my eyes, have been invaluable. May that which I am now going to say be commended to you at the same time as homage paid to the memory of the discoverer of the sarcous elements and of the waves of contraction.

The subject of my lecture is an inquiry into "The Nature of Muscular Contraction." Like all vital phenomena, muscular contraction is a most complicated process, composed of mechanical, chemical, thermal, and electrical changes in living matter. Hence it will be our task to become acquainted with these changes as completely and exactly as possible, and to ascertain their causal connection. Our inquiry must not be restricted to one special kind of muscle: it will have to extend to all the different forms, the highest as well as the lowest—muscles in every stage of development, muscles either sound or unsound,—for there can be no doubt but that in all these cases the principle of activity is the same. Nay, it will be necessary to deal even with the other phenomena of so-called contractility, such as protoplasmic and ciliary motion, for all those different types of organic movement, however much they may differ from each other in details, are yet connected by states of gradual transition, so that, to all appearance, one principle of motion, essentially the same, is applicable to all of them.

Only such properties and processes as all contractile structures have in common will consequently have a right to be considered essential to the process of contraction, and only such will be allowed to form the basis of our endeavours to explain muscular motion.

The general mechanical principle on which muscular contraction is based, will apparently be discovered when we shall have ascertained in what way the power of shortening proceeds from the potential chemical energy which disappears upon stimulation of the muscle. There can be no doubt as to the fact, that the potential chemical energy of the component parts of muscular substance is alone the ultimate source of this power, for the existence of any other source cannot be proved.

The quantity of energy which is imparted to the muscle by the stimulus is too small to be taken into consideration. The early opinion that the power required for contraction was imparted to the muscle through the medium of motor nerves, was refuted by experiments, such as, *e.g.*, those on the persistence of contractility after degeneration of the motor nerves, and on the effects of direct artificial stimulation of the muscles; and it had even been refuted long ere the law of conservation of energy had thrown its light on the mutual connexion between the phenomena of the living organs.

This law teaches that all the actual energy which appears in the muscle in consequence of stimulation, must originate in an *equivalent* quantity of some other form of energy.

Now this form of energy is, in fact, given in the muscular substance liable to physiological combustion. The quantity of the latter is not only theoretically sufficient to produce that actual energy, but it has even been proved experimentally that during contraction the material gives rise to combinations, such as carbonic acid, in the development of which potential chemical energy must have passed into other forms of energy. So far as the phenomena have been examined *quantitatively*, they confirm the conclusion that all muscular force must be derived from chemical energy.

Hence there is no difference about all these points. But with this result we have as yet gained only a basis for the proper solution. As soon as you inquire in what way, by what transformations, does the mechanical force of contraction arise from chemical energy, difficulties and differences of opinion begin to present themselves.

A grêat many physiologists hold, with Pflüger, Fick, and Chauveau, that muscular force is a direct manifestation of chemical attraction; others, *e.g.*, Solvay, think that it is produced through the medium of electricity; others again, following J. R. Mayer, believe that the muscle is a thermodynamic machine, not unlike our caloric or steam engines.

The Chemiodynamic Hypothesis.—The first hypothesis, according to which contraction of muscle is a *direct* manifestation of chemical attraction—we may call it the chemiodynamic hypothesis—has to assume that the molecules, on the chemical combination of which this contraction is based, are regularly arranged within the contractile substance in such a way that they necessarily approach each other, during that combination, in the direction of the axis of the muscular fibres.

I think that this hypothesis of the identity of chemical attraction and muscular force meets with a fundamental difficulty in the fact that, in a single contraction, only a relatively infinitesimal part of the muscular substance is chemically active. 70 to 80 per cent. of the muscle (and even more) consists of imbibed water, the rest contains substances (albumin, salts, &c.) which, for the greater part, so far as can be proved, are not chemically concerned in the contraction.

This quantitative composition and this minute consumption of the active muscle compel us to assume that relatively only very few molecules of the muscular substance can be considered as sources of energy, and of these again it is generally but a small part that at a certain moment perform their function. How else were it possible for a severed bloodless muscle to continue to carry out, without any perceptible decrease of weight, many thousands of contractions, and how else could a frog's heart, isolated and all but bloodless, beat on for days together, with a nearly constant frequency and but slowly sinking power?

With certain presuppositions we may compute the quantity of matter through the chemical action of which the amount of actual energy, produced at a certain contraction, must have been generated.

If we prevent a muscle from doing external work during the contraction, the whole actual energy will present itself in the shape of heat. When there is but a slight contraction, the muscle of a frog, *e.g.*, will grow warmer by about 0·001° C. Supposing the specific heat of the muscle to be equal to that of water (in fact it is less), we find that to produce a rise of 0·001° C. in temperature a quantity of heat of 0·001 cal. is required for each gram of muscle. No matter whether this quantity of heat results from combustion of carbohydrates, fats, or albuminous matter, it can be but an infinitesimal part of the muscular substance that produced it. If, *e.g.*, as is ordinarily supposed, the combustion of a carbohydrate into CO_2 and H_2O produced that heat, taking the heat of combustion of one gram of carbohydrate to be broadly 4000 cal., no more than a four thousandth part of a milligram will have been consumed in each gram of the muscle. Hence only about a four millionth part of the muscular substance could have been the source of the actual energy, set free by the stimulus, and have served at the same time, according to the above hypothesis, as the seat of direct attraction.

But whatever may be our conception of the size, form, position, and sphere of action of this four millionth part with regard to the rest of the soft, watery mass, only passively moved, I fail to understand how, through *direct chemical attraction*, this one particle should set all those other four million parts, less one, moved, as in fact it does.

We must not forget that we have to deal with enormous changes of form. Muscles of insects are sometimes shortened by local contraction to one twentieth of their length during rest. The myopodia of *Acanthocystis* can be reduced to a fiftieth part of their length by the action of even a weak stimulus; in this case an extremely long, thin fibre is in a moment changed into a short cylinder, about as broad as it is long.

That this can ever be brought about by direct chemical attraction, seeing that but a relatively infinitesimal part of the soft watery substance of the fibre is the seat of this chemical attraction, seems to me difficult to understand, even if we assume—as I believe we must do—that the *number* of the chemically active molecules is, even in the smallest visible particle, so large that the distances which separate them from each other and from the passively moving molecules lying between them, fall within the limits in which molecular mechanical influence can be exercised.

The adherents of the chemio-dynamic hypothesis have not as yet answered this objection. And since they can give but an unsatisfactory account or no account at all of many other facts (I will refer to some of these facts further on), we may be allowed to cast about for some other explanation.

The Electrodynamic Hypothesis.—Since Galvani's discoveries the electric phenomena of the muscles have frequently been suspected to contain the solution of our problem. And indeed, it is not so very difficult to mention a series of facts which seem to bear out the suggestion that the mechanical work done by the muscle may be created from chemical energy through the medium of electric forces.

There is, in the first place, the fact that muscles, when in action, produce regular electric effects. These effects are indeed the first phenomena we can observe after the stimulation. They seem to begin at the very moment of stimulation, shortly before the contraction; hence they might in so far be the cause of the mechanical process.

Moreover, the value of the electromotive force, as du Bois-Reymond proved, is very high, and in the active particles is probably much higher than the force of the currents we can derive from the surface of the muscle.

Add to this that the economic coefficient of the muscle may attain, just as in the case of electric motors, a considerable proportion. As much as 25 per cent. and more of the potential energy which has been consumed may be transformed into mechanical work.

However, there are weighty objections to this hypothesis also. In the first place, there is the fact that these very same electromotive forces, of equal intensity and direction, appear, under the same influences, not only in the muscles, but also in nerves, glands, and other organs, which do not possess the least contractility. Then there is the important discovery of Biedermann, that the contractility of muscles may be completely neutralized by water or ether vapour, without doing any perceptible harm to the electromotor phenomena.

In the same way the development of the electric organs supplies us with important proofs of the independence of the electric and the mechanical processes. In most cases these organs are developed out of striped muscular fibres. Now, in this process of development, contractility is gradually lost, whereas the power of producing electrical effects attains a yet higher degree of perfection.

The Thermodynamic Hypothesis.—More probable than the chemical and the electrical hypothesis may be deemed a suggestion, first put forward by Jul. Rob. Mayer, though in an untenable form, according to which the muscle is a thermodynamic machine. Physiologists, however, generally object that this view is not compatible with the second law of the theory of heat, for we cannot expect differences in temperature in the muscle so great as this law requires they should be.

Now I think that, on the contrary, we must assume exceedingly large differences of temperature in the stimulated muscle. What holds good of the whole body holds good of the muscle also; the temperature, measured with our instruments, is but an arithmetical average, "comprising an infinite number of different temperatures, pertaining to an infinite number of different points" (Pflüger).

From the fact that at the contraction an infinitesimal part only of the muscular mass is chemically active, we infer that the temperature of these particles must, at the moment of combustion, be an uncommonly high one. Great as the specific heat of muscular substance is, it would otherwise be impossible to account for a rise in the temperature of the whole mass even of 0·001° C. only. Without any exaggeration we may assume that the temperature of the chemically active particles may, at the moment of combination, exceed the average muscular temperature by hundreds of degrees.

Since each thermogenic particle is surrounded by a relatively enormous cool mass, conducting heat and diathermanous, the principal condition for the transformation of heat into mechanical work has been satisfied, and, on account of the enormous differences in temperature which we have to assume, in such a high degree, that even an economic coefficient of 30 per cent., nay, 50 per cent., and even more, seems to be theoretically possible.

Supposing we have to deal with a Carnot's cycle, the theoretical maximum Q_0 of the mechanical effect is $Q_0 = Q \frac{T_1 - T_2}{T_1}$, where Q stands for the whole quantity of heat, which from the absolute temperature T_1 is sinking down as far as T_2. Taking $T_2 = 273° + 37° = 310°$, the mechanical effect might at $T_1 = 410°$ amount to 25 per cent., when the temperature of the active particles would consequently exceed the average temperature of the normal muscle by 100° C. only.

The objection that these high temperatures must necessarily destroy the life of the muscle, since the latter becomes rigid and dies even at 50° C., is, for the same reasons, of small value only. For it is ever an infinitesimal part only of the muscular mass that is exposed to these high temperatures. At a small distance from these furnaces of heat the temperature must have fallen so low as to be harmless. The muscle will no more be destroyed by stimulation than a steamship will be destroyed by heating the furnaces. The material of combustion only will be destroyed; the vessel as a whole remains unharmed.

However likely it may thus seem that nature should avail herself of these favourable terms on which mechanical work may result from muscular heat, we have had up to the present time no direct proof that this is actually the case, nor do we know in what way it takes place, if in any. But I venture to think that the proof can now be given, inasmuch as it is possible to demonstrate how, through the medium of peculiar arrangements of the material of the muscle, a transformation of chemical energy into mechanical work by means of heat not only can, but actually must, be brought about.*

Muscular Structure in relation to Contractility.

The Fibrils are the Seat of the Shortening Power.—For this we need firstly to pay attention to the peculiarities of the microscopical structure of muscle. All muscular fibres of all animals are composed chiefly of two parts: extremely thin, long, albuminous fibrils, and an interfibrillar plasmatic substance, the so-called sarcoplasm. The quantitative relations of both vary, but the fibrils always occur in great number, forming very often the greatest part of the whole mass of the muscle. They always run parallel to each other throughout the length of the fibres.

This fibrillar structure is also presented by all the other formed contractile substances, such as the myonemata of Infusoria and of other Protozoa, the cilia and vibrating membranes of cells, the tails of spermatozoa, nay even, at least, when not in a state of contraction, by the amœboid protoplasm of *Pelomyxa*, *Myxomycetes*, and other forms.

Direct microscopical observation during life teaches us that the fibrils, and not the sarcoplasm, are the seat of the shortening power. The fibrils in a state of relaxation are long and thin, and often run in winding curves, but grow short, thick, and straight, in consequence of stimulation. The sarcoplasm passively follows their movements. Moreover, completely isolated fibrils can shorten.

The Fibrils are Contractile because they contain Doubly Refractive Particles.—Thus the question arises: can there be demonstrated in the fibrils arrangements such that by means of them contractile force may originate in a thermodynamic mode?

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* The empirical foundations of the views developed in this lecture will be found in "Versuche über Aenderungen der Form und der elastischen Kräfte doppelbrechender Gewebselemente unter chemischen und thermischen Einflüssen," in the Appendix of my Memoir: 'Ueber den Ursprung der Muskelkraft' (2te Auflage. Leipzig. 1893. Pp. 54—80), and in the literature cited in the same paper.

Light—*lux optimum reagens*, as Buys Ballot said—solves this question for us. If we examine the optic properties of contractile fibrils, with the aid of the polarising microscope, we find that all of them are double-refractive, with one optical axis parallel to the direction of contraction. This property, long ago discovered by Boeck in the striated muscles of the higher animals, has now been proved to appertain to the muscular fibres of all the animals examined for that end, to the contractile fibrils of Protozoa, *e.g.*, to the muscle of the stalk and the myonemata in the subcuticular layer of Vorticella, to all kinds of ciliary apparatus, and in many cases to contractile protoplasm also.

This general occurrence of double-refracting power is the more indicative of relations to contractility, since non-contractile cells, as a rule, lack double refraction, even where we meet with a fibrillar structure, as in the axis-cylinder of a nerve-fibre.

Our conjecture gains, I believe, a very high degree of probability by the following series of observations, which are the more convincing since they relate to quite different categories of phenomena, independent of each other.

In the first place, there is the fact that contractility and double refraction in the course of ontogenesis always appear at the same time, *e.g.*, in the heart of the chick on the second day of incubation; in the muscles of the trunk and skin on the fifth or sixth day; in the muscles of the tails of tadpoles when the length of their body is 3 to 4 mm.; in the muscles of the stalk of Vorticella, and in cilia so soon as these organs become visible.

Other evidence seems to me to be afforded by the behaviour of the striated muscles. Here the fibrils consist of the doubly-refractive sarcous elements and the singly-refractive material which joins these, the two alternating regularly. The two are wholly different as regards their optical, mechanical, and chemical properties; and these properties, moreover, during contraction, change in an opposite way. Hence the functions of the two must be different. And since the changes of form, volume, &c., of the doubly-refractive parts during contraction prove that in each case these parts must be the seat of contractile power, the singly-refractive junctions will most probably have another function. We will come back to these changes further on.

A third evidence is afforded by the observation that the specific force of contraction in different muscles is, in general, greater, the better developed the power of double refraction, comparison, of course, in each case being made with parts of the same thickness.

In the development of the pseudo-electric organs of *Raja* out of striated muscular fibres, one of the signs of the incipient change of structure and function is the vanishing of double refraction in the sarcous elements. In an early stage of development this vanishing is, with *Raja clavata*, the very first and the only sign that the fibre is about to be transformed from a contractile into an electric organ.*

But particularly significant seems to me to be the behaviour of the obliquely striated muscles of Molluscs and other Invertebrata. Here the doubly refractive fibrils do not run parallel to the axis of the fibre, but describe spiral lines round it; and during a contraction the steepness of the curves decreases, so that the angle formed by the longitudinal axis of the fibril and the longitudinal axis of the fibre may increase from 5° in the relaxed state to 60°, and even more, in a state of powerful contraction. But the optic axis of the fibril, instead of assuming, in this case, a more oblique position also, as might be expected on morphological grounds, remains parallel to the longitudinal axis of the fibre, and consequently to the direction of shortening of the fibre. Hence it is not the morphological axis of the fibrils, but the optical axis of their doubly refractive constituents, which coincides with the direction of the contracting force.

Contractility a General Property of Doubly Refractive Bodies.—More than a score of years ago I pointed out the fact that even non-muscular elements, elements not possessing irritability in the physiological sense of the word, nay, even lifeless, unorganised elements which are doubly refractive and uniaxial, may, under certain influences, contract in the direction of the optical axis, all thickening at one time, and contracting with a force and quickness and to an extent rivalling that of muscles, if not surpassing it. Instances of this are the fibrils of connective tissue, of the tendons, and of the cornea and others. The same contractile power was found by von Ebner in a great many other doubly refractive histological elements, nay, even in substances capable of imbibition and thereby made doubly refractive, *e.g.*, dried colloid membranes; and finally by Hermann in fibrils of fibrin.

I have in this way shown that singly refractive, or only feebly doubly refractive histological elements, such as fibres of elastic tissue, in the same way as caoutchouc, obtain the power, when made doubly refractive by extension, of contracting under certain influences, and further that the force of shortening will generally be greater in proportion to the amount of the double refraction thus artificially produced.

Since, according to Mitscherlich's discovery, similar changes of form may be observed in doubly refractive crystals, we have apparently to deal with a property pertaining to all doubly refractive bodies as such.

Heat as a General Cause of Contraction of Doubly Refractive Elements.—Now, the influence which in all these cases is able to evoke the mechanical energy of shortening is *elevation of temperature.* Cooling has the opposite effect.

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* Th. W. E., "Die Blätterschicht der electrischen Organe von Raja in ihren genetischen Beziehungen," &c., 'Pflüger's Archiv,' Bd. 57, 1894, p. 149.

Particularly instructive is the thermal contraction of fibrillar connective tissue, on account of its similarity to muscular movement, even with regard to details. This tissue, like muscle, consists of doubly refractive fibrils and of an inter-fibrillar singly refractive isotropic substance. The fibrils have about the same dimensions as muscular fibrils, and, for the rest, show, in their optical and mechanical behaviour under various influences, a great likeness to muscular fibrils, especially to the doubly-refractive sarcous elements.

In tendons and many membranes the fibrils, as well as those of most muscles, are arranged into bundles, all, or nearly all, parallel to each other. For this reason such objects are extremely well fitted for a closer examination of the phenomena of movement. The most suitable material I know is furnished by the catgut string of a violin, which consists chiefly of such bundles, running in steep spiral lines round the longitudinal axis of the string. Such a string is distinguished from the greater number of naturally occurring objects by its very regular cylindrical shape and its elasticity. On these properties is based its suitability for musical purposes, especially for the so-called "perfect fifth" ("Quintenreinheit").

The Muscle-Model.—With the aid of such a string we can compose a model which in a simple way explains how in the muscle mechanical energy of contraction may result from heat without any perceptible rise of the average temperature of the muscle.

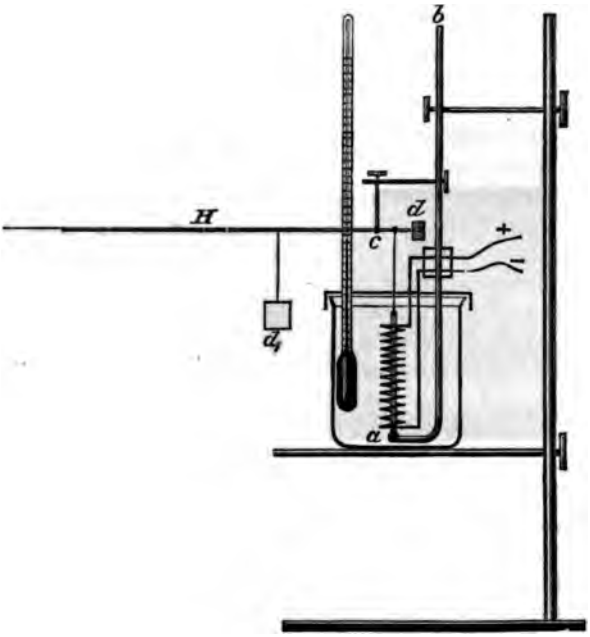
The following sketch (fig. 1) presents this *model of the muscle* diagrammatically simplified:—

A piece of an *E* string of a violin, about 5 cm. long and previously swollen in water, is fastened to the end, *a*, of the short rigid arm of a steel rod, *ab*, while the upper end of the string is fixed on the shorter arm of the lever, *H*, turning round the horizontal axis, *c*.

To this string different tensions may be imparted by weights (d, d_1), or springs, acting upon the lever, and since the point of application of the string may be changed at will, its contraction, with all the conditions of isometry, isotony, and auxotony may be examined.

Round the string, but without touching it, runs for a length of about 20 mm., and in about twenty curves, a spiral of thin platinum wire. Its ends may be connected by means of two binding-screws of the ebonite or ivory piece, *f*, with the two wires coming from the poles of a Grove or Bunsen battery of three or more cells. The rod, *ab*, bearing lever, string, and spiral wire, is placed in a glass of about 50 c.c. content, filled with water of about 55—60° C., and closed at the top by an ebonite lid. Through an aperture in the lid, a thermometer is placed in the water in such a position that it will remain at a distance of about 1 cm. from the spiral wire.

FIG. 1.



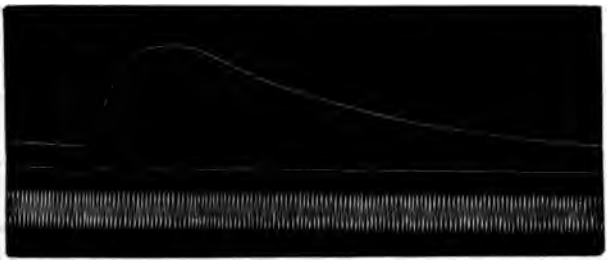
The string is now observed for some minutes at a tension of 25 or 50 grammes, and at a constant temperature, until no further change in the position of the lever can be discerned. If we now close for some seconds the circuit of the battery through the spiral, *the lever rises. Upon opening the circuit, it falls. The thermometer in the glass indicates a hardly perceptible rise in temperature, or no rise at all.*

We see the doubly-refractive string of our model corresponds with the doubly-refractive muscular particle, which we suppose to be the seat of the force of contraction, and therefore may be called "*inotagma;*" the water in the glass represents the watery isotropic substance round the inotagma, doing duty as refrigerant; the spiral wire supplies the place of the chemically active *thermogenic* molecules; the closure of the galvanic circuit corresponds with the process of the stimulation of the muscular element.

The experiment may be simplified by heating in the air a string previously made to swell by being dipped in hot water and then soaked in concentrated glycerine. The movements will then take place more quickly on account of the quicker heating and cooling. Moreover, the experiment may be repeated almost as often as one likes. If we fasten to the lever a little metallic hook, which when sinking closes the circuit of the spiral by dipping in mercury, and breaks it again when rising, contraction and relaxation of the string will alternate regularly for hours, just as with a beating heart.

The movements may be inscribed on a rotating cylinder. We then obtain curves of the same character as contraction-curves of muscles (fig. 2).

FIG. 2.



Such a *chordogram* presents, like a myogram, three periods, viz.:—

1. A period of latent energy, the duration of which, just as with the muscle, decreases with the increasing energy of the stimulus (*i.e.*, with the intensity and duration of the electric current), with rising temperature and with decreasing load.

2. A period of augmenting energy, in which contraction takes place with a rapidity, first increasing, afterwards diminishing, the contraction being, within certain limits, more rapid and larger in extent the stronger the stimulation.

3. A period of declining energy, in which the string relaxes with a gradually decreasing rapidity.

Further Comparative Researches on the Thermal Contraction of Lifeless Doubly Refractive Bodies and the Physiological Contraction of Muscle.—The points of resemblance between our model and a muscle extend much further yet, and amongst other to peculiarities which seem to bear important testimony to the identity of the mechanical process in the two cases.

Such a resemblance I find, in the first place, in the fact that the strength of *the shortening power, developed by a certain stimulus, increases with the load* within certain limits. Both muscle and string present the paradoxical phenomenon that, under a stimulus of equal energy, heavier weights may be lifted higher than lighter ones.

Adolf Fick first detected this surprising fact in the muscles of *Anodonta;* and Rudolf Heidenhain, almost at the same time, observed it in the striated muscle of Vertebrata. It has been generally confirmed, and seems to hold good for all kinds of muscles.

Neither the chemical nor the electrical hypothesis of the origin of muscular force can give a sufficient explanation of this fact. On the basis of our theory, on the contrary, it can be predicted, because every influence which augments the doubly-refractive power must raise the power of contraction.

Now, von Ebner has proved experimentally that the doubly refractive power of tendons and also, between certain limits, of muscles, increases with the load. The same is the case with fibres of elastic tissue and with caoutchouc, and here also the contractile power increases with the load.

The differences of force which depend on the load are by no means insignificant. According to Fick the lifting heights for 5, 8, and 10 gr. were, with a muscle of *Anodonta*, related to each other as 8·3 : 10 : 16·3. With *E* strings, swollen in hot water, I obtained, when heating them from 20° to 80° C., at an initial load of 5 gr., a contractile power of but 55 gr.; at a load of 50 gr. one of 80 gr.; and at a load of 90 gr. one of about 120 gr.

For *E* strings swollen in lactic acid of 0·25 per cent. the rates of power, corresponding with the initial tensions of 5, 90, and 125 gr., amounted to about 100, 125, and 154 gr. A strip of elastic tissue, 2 mm. thick, heated at 80° C., gave for an initial tension of 10 gr., at which the double refraction of fibres is just beginning to be perceptible, a power of but 61 gr., for 100 gr. one of 162 gr., for 185 gr. of initial tension, at which double refraction is very strong, one of 238 gr. A strip of vulcanised caoutchouc, 2 mm. thick, which, unloaded and hence singly refractive, did not, even when heated to 100° C., tend at all to become shorter, developed with an initial load of 90 gr., put on at 20° C., a contractile power of 110 gr. at a temperature of 80° C., while at the same time a strong power of double refraction had been acquired by it.

Now, it is true, that Heidenhain has found that—at least in the stimulated *striated* muscles—not only mechanical energy but also the development of heat and the formation of lactic acid increase together with initial tension. But, according to his experiments, mechanical energy increases much more rapidly than does the production of heat. This difference at least might be explained by the influence of extension on the doubly-refractive power. An influence of extension on chemical action may very well be associated with the influence on double refraction, as a consequence of the changes in the local arrangement of the thermogenic particles which are produced by the extension. Such changes must be effected to a considerable degree, even when the shortening of the muscle is prevented by resistance (isometric contraction of Fick), and especially in the case of striated muscles, to which Heidenhain's statements refer. For not only fibrils, interfibrillar substance, and sarcolemma, but also the singly and the doubly-refractive portions of the fibrils differ much in elasticity. It would be very strange if these shiftings did not manifest themselves also in changes of the chemical processes, and if these changes were not conformable to the purpose.

Connected with the point we have just discussed is another fact, viz., that the amount of shortening produced in our model by means of a given rise of temperature, is the smaller the more the string has already contracted. The maximum of force is, at all events, displayed when the extension of the string is brought about by the whole load being applied at once at the very beginning of the heating, not after the string has already contracted with a smaller load.

The very same thing, as Schwann's experiments showed many years ago, holds good of muscle. On the hypothesis of chemical attraction we should decidedly expect the reverse: viz., increase of force with an increasing mutual approach of the combining molecules; so also in the same way on every hypothesis which pronounces contraction to be caused by attractive powers increasing in inverse ratio to the square of the distance.

In the fact discovered by Schwann, Johannes Müller thought he had found a refutation of the old electro-dynamic hypothesis of Prévost and Dumas, as well as a valid reason for assuming a fundamental relation between the vital power of contraction and physical elasticity.

However, as Hermann has observed, we might in this case get over the difficulty by supposing that between or in the length of the parts attracting each other, there are elastic layers opposing that attraction with increasing force. It is evident that our view of the matter does not require such an auxiliary hypothesis, because, following Eduard Weber, we regard muscular contraction as only a special case of elastic shortening.

A closer experimental comparison of the changes undergone, on the one hand, by the elasticity of our string during thermal shortening, and, on the other, by muscular elasticity during physiological contraction, will teach us that, in each case, those changes are of exactly the same kind.

As regards striated muscles, it was Eduard Weber who, by his classic researches, established that their extensibility increases during contraction. The same is now proved to hold good of strings and other organic doubly-refractive substances during thermal shortening.

The curve of lengthening of all these objects inclines more sharply towards the abscissa of the loads the higher the temperature. Both curves converge and may finally even cross, *i.e.*, a certain load being exceeded we do not get contraction of our string, but lengthening as the effect of heating.

This circumstance explains the fact, sometimes observed by E. Weber, that living, tired, heavily-loaded frogs' muscles, lengthen instead of shorten as a result of electric stimulation. Considered from other theoretical points of view, this observation seems so paradoxical that its very validity has been questioned by some physiologists; but in the face of the direct and exact measurements of so scrupulous an observer and inquirer as Eduard Weber, we have no right to do this. Moreover, Charles Richet and de Varigny have confirmed the observation with different kinds of muscles of Invertebrata. According to our view of the origin of muscular force the fact is not paradoxical at all, but might be foreseen.

The decrease of the shortening power and the increase of extensibility with increasing thermal contraction is, in the case of our lifeless doubly-refractive objects, accompanied by a decrease in the power of double refraction. According to von Ebner's careful measurements, confirmed by Alex. Rollett, the same thing is the case with muscles during vital contraction. We may consider this fact, too, as an important proof of the fundamental resemblance between the process of contraction in our model and in the muscle, and at the same time as a further evidence of the existence of a causal relation between double refraction and contractility in general. But it is the physicist's task, and not the physiologist's, to penetrate further into the relations between optic and elastic properties. The physiologist may deem his purpose attained when he succeeds in tracing a certain vital phenomenon back to processes which may also be observed in lifeless bodies.

However, though we should, perhaps, be inclined to infer from the foregoing that we have successfully acquitted ourselves of this task with regard to muscular contraction, we will be careful not to overlook the numerous important respects in which a muscle as a *living* body, that is, one subjected to constant chemical transformation, differs from our lifeless strings. The study of these differences is most instructive, since it throws a new light on a series of processes nearly allied to contraction, especially on the phenomena of *rigor mortis* and *tonus* of muscle.

But before entering into this we shall first have to meet another important objection to our views. It is based upon the absolute amount of muscular force. This amount may, as you know, be very high. Human muscles at the strongest tetanic contraction can shorten with a force of about 10 kilogrammes to 1 sq. cm. of transverse section. Now such a force must, according to our view, be produced by a very small part only of the transverse section of the muscle.

With a maximal tetanus, it is true, the temperature of the whole muscle does rise 1° C. or more. Hence there are, perhaps, 1,000 times more particles chemically active than with a moderate simple contraction, where the temperature rises 0·001° C. only. Consequently, during such a tetanus, a much greater part of the muscular substance —perhaps 1,000 times as much—will be heated to such a degree as is required for an obvious contraction of the inotagmata. But even in this case the greater part of the whole substance will be only moved passively.

This holds good also, though in a less degree, of the single fibrils or of the single sarcous elements, which, on account of their containing doubly refractive inotagmata, we believe to be the sole producers of shortening power. For it is beyond question that the sarcous elements *in toto* are not subjected, even during the strongest tetanus, to a temperature so high as is required by our hypothesis. Hence the specific force of contraction of the individual doubly-refractive elements must be comparatively much greater. We cannot properly calculate *how much* greater the absolute force of a muscle would be if the transverse section consisted of nothing but contractile inotagmata, we can only assume that it would, in general, be many times as great.

Can such very important mechanical powers as we are obliged to assume in the inotagmata be developed through the thermal contraction of doubly-refractive bodies? Do we not, as Fick says, go beyond the bounds of legitimate analogy in making such a supposition?

Of course nothing but the measurement of the forces developed by lifeless doubly-refractive bodies under thermal contraction will decide this question. I have made many of these measurements on various objects, and I think the results furnish us with a refutation of the objection. Strings, moist but not yet contracted through lying in water, with a diameter of 0·7 mm., and loaded with 1 kilogramme, lifted up the weight in a perceptible degree when rapidly heated up to 130° C.; that is to say, they exerted a force about twenty times at least as great as the maximum force of a human muscle of the same thickness. This is all the more striking, since in strings also it is not the whole transverse section, but the sum of the sections of the fibrillæ, and even a part of every section only, which is the seat of the power of shortening.

Still greater forces may be exerted by strips of caoutchouc rendered in a high degree doubly refractive by strong extension. Even merely heating from 20° to 40° C. produced results sixty times as great as the maximum afforded by human muscles of the same transverse section.

Hence we may sufficiently account for the greatest display of force of which muscle is capable, without having to attribute to the inotagmata higher elastic forces than we observe in highly extended threads of caoutchouc of the same thickness, nay, without even having to assume temperatures reaching the degree necessary for the coagulation of albumin.

Influence of Heat on the Dead Muscle Fibres.—It is a pity that we are not able to subject the isolated doubly-refractive parts of the muscle in an unimpaired condition to the influence of heat. Together with the elevation of temperature there occur changes in the chemical processes, and therewith in the material composition and mechanical properties of the whole muscle substance, which complicate the changes dependent only on the heating of the doubly-refractive particles, or even prevent our clearly recognising them.

We may, it is true, exclude these chemical influences by previously killing the muscles by means of alcohol or some other medium, the action of which preserves the finer structure of the non-contracted muscle, and above all the double refraction. But in this case, too, the mechanical properties of the fibrils, as well as of the sarcoplasm, change so considerably that the explanation of the phenomena becomes very difficult. A great part of the soft muscular substance becomes solid by coagulation or precipitation, and these solid parts will act in opposition to a thermal shortening, nay, perhaps compensate it, in consequence of their own expansion through heat. Moreover, the phenomena are rendered complicated by the thermal changes of other elements of the muscular tissue, such as connective fibrils, blood-vessels and nerves.

The results, however, agree very well with the hypothesis that even in a dead muscle the doubly-refractive particles tend to contract when heated to a certain degree, and to lengthen again when cooled. The degree of heat at which quite dead, lightly loaded frogs' muscles begin to shorten, does not lie much higher than those sufficient to produce rigor, mostly below or near to 60° C. Contraction increases with the temperature, occasionally amounting to 20 per cent. of the initial length.

Very important contractions of the sarcous elements—to the half of the length and more—are shown by the microscope in the striated muscles of insects, when these are very rapidly heated to 55°, or higher. The singly-refractive layers do not contract, or at least to a much less degree. However, we will not attach too much importance to these facts, because the conditions are very complicated.

Tetanus and Rigor by Heat.—Living muscles, when being *gradually* heated, are, as you know, thrown into tetanus so soon as the temperature has attained the height of a little below 50° C. This so-called *tetanus of heat* passes by prolonged heating into the lasting contraction of *rigor*, accompanied by definitive loss of irritability.

This contraction through heat agrees in so many points with physiological contraction, especially with physiological tetanus, that it was long held to be the last manifestation of muscular life. Such points of resemblance are, *e.g.*, the amount and the force of shortening, which in both cases are at least of the same order, and the increased production of heat, of carbonic acid, and of a fixed acid.

No doubt in this case a very important and general rise of temperature of the contractile particles will take place so soon as rigidity begins to announce itself. Consequently, according to our hypothesis, we must expect a strong and general contraction of the inotagmata.

That the force, with which the muscle as a whole will shorten, is not quite so great as with physiological tetanus, is sufficiently explained by the fact that the inotagmata do not contract simultaneously, and by the increase of internal resistance which occurs, due to coagulation and precipitation in the muscle plasma during the development of rigidity by heat. The latter circumstance seems to explain, too, why the rigid muscle does not perceptibly, or only very little, lengthen upon cooling.

Turgescence by Imbibition as a General Cause of Contraction of Doubly-refractive Organised Elements.—On a closer examination, however, we find that matters are still more complicated, and likewise that there is still an important circumstance which, besides the rise of temperature of inotagmata, may act as a cause of contraction, even of permanent contraction. This circumstance, the fundamental importance of which to muscular contraction was disclosed a score of years ago by a rigorous microscopical examination of the processes taking place in the muscle fibres during contraction, is the *turgescence of the doubly-refractive elements by the imbibition of watery liquids.*

All histological elements possessing doubly-refractive power tend, even at an ordinary low temperature, to contract in the direction of the optical axis when their volume is enlarged by the imbibition of a watery fluid, and to lengthen when their volume diminishes by loss of liquid. The extent, power, and rapidity of the changes of form depend on the nature and on the dimensions of the turgescent object, and on the nature and quantity of the imbibed liquid.

For the examination of these relations our violin strings again yield fit material. A long series of measurements has now shown that there is a very close resemblance between contraction by imbibition and thermal and physiological contraction. I may mention the marked extent of the shortening, the high value of the force of contraction, its increase with the initial tension and its decrease with increasing shortening, the increase of extensibility, the decline of refractive power and of doubly-refractive property. The resemblance is by no means exclusively of a qualitative, but also of a quantitative kind.

A change of form generally takes place when the composition of the imbibed liquid changes, and it is of great importance to our question that even the slightest changes of composition can cause marked contractions and great mechanical effects.

Unloaded *E* strings, *e.g.*, contract in pure water to nine-tenths, and in water which contains 0·25 per cent. only of lactic acid to threefifths of the initial length. At 15° C. they exert, in the first case, forces of about 80 gr., in the second of about 110 gr. With initial tensions of 105, 335, and 450 gr., forces of 226, 365, and 496 gr. respectively were produced by swelling in distilled water at 15° C.; and by imbibing a 0·25 per cent. solution of lactic acid at initial tensions of 5, 215, and 425 gr. forces were exerted of 115, 350, and 490 gr. respectively, *i.e.*, forces very much higher than a muscle of the same thickness can produce during tetanus. Even in a watery solution of only 0·1 per cent. of lactic acid, the strings contract to about 70 per cent., as also in highly-diluted acetic and hydrochloric acid or in potash.

Upon neutralisation or dilution the old length and volume return. The doubly-refractive fibrils, or the sarcous elements of muscles, contract considerably, also under the same conditions, swelling at the same time; this is the case even with muscles which have been killed in alcohol. In such instances I measured in the striated fibres of insects, distinguished by especially long sarcous elements, shortenings to 50 per cent. and more.

Since, according to many inquirers, lactic acid is formed during the rigor of striated muscles, and at all events the reaction of the muscular plasma grows acid, the doubly-refractive elements must necessarily swell more and tend to shorten, and this contraction will remain until the acid has been neutralised or removed by diffusion.

Similar results will be obtained in other cases of rigor characterised by shortening and by the production of much acid, such as the ordinary rigor mortis, the rigor produced by distilled water, the rigor which is caused by excessive stimulation, &c. Nay, in a bloodless muscle even a physiological stimulation, when sufficiently strong and long, may be expected to produce a lasting shortening, on account of the gradually increasing acidity. Indeed, the well-known incomplete relaxation of such muscles seems to me to be a symptom of this *chemical contraction*, as it may be called, in contrast with the *thermal*.

In a muscle in which the blood stream is maintained this will not so easily take place, not even under a strong and prolonged stimulation, because the acid is immediately neutralised or removed through diffusion. Even in the isolated, bloodless muscle the acid, which is produced by stimulation, may, in the beginning at least, be rendered harmless through the very large quantity of non-acid fluid imbibed by the muscle. Consequently we must expect in these cases an immediate and complete relaxation after contraction. The facts agree absolutely with these suppositions.

It is, perhaps, not unnecessary to remark that all these observations would also hold good if the material affecting the turgescence were not lactic acid, but another substance arising during the chemical action in the muscle, *e.g.*, water.

The different parts played by "Thermal" and by "Chemical" Contraction in the different kinds of muscular contraction.—But now the question may be raised: Is not physiological contraction due to turgescence by imbibition solely? Do not we find a sufficient explanation of all kinds of contraction in the change of the conditions of imbibition, a change which must necessarily result from the chemical action caused by the stimulus?

We have all the more reason to put this question, since we can prove, that in the physiological contraction of striated muscle-fibres the doubly-refractive layers swell at the cost of the watery isotropic layers. The microscopical examination of active living muscles and of fixed waves of contraction has proved this fact beyond all question, however much the opinions of different observers may diverge on other points. The swelling would, moreover, account for the slight decrease of muscular volume observed in strong tetanic contraction. For, according to the experiments of Quincke, the imbibition of water by organised bodies generally leads to a slight condensation.* By this condensation further heat is developed, and this heat might, by raising the temperature of the doubly-refractive elements, be partially transformed into mechanical energy, and in this way contribute to the production of muscular force.

Yet I cannot consider this explanation as sufficient for all the facts. The same argument which in our eyes seems to dispose of the hypothesis of the identity of chemical attraction and muscular force, viz., the infinitesimally small quantity of substance which is chemically active during a simple contraction, seems to me to present a fundamental difficulty here also. It is hard to understand how through a change in the material composition, effected at one infinitesimal point within a soft watery substance, the whole mass should shorten and thicken, *unless there proceeds from the centre of chemical activity a considerable amount of kinetic energy throughout the substance.*

The microscopic appearances which prove the turgescence of the doubly-refractive refringent elements during a contraction, do not exclude a direct thermo-dynamical effect. For the almost complete identity in the changes of form, and of optical and mechanical properties, which the doubly refractive constituents of all histological elements undergo during chemical and thermal contraction, seems to bear out the hypothesis, that in the thermal shortening of doubly-refractive elements, through the imbibition of watery fluid, we get a shifting of solid and liquid substances analogous to that of turgescence. In most of the microscopical phenomena, especially the so-called fixed contraction waves, we have, moreover, to do with a high degree of tetanic contraction, or even with rigor, in which, on account of the greatly-increased chemical action, a chemically-caused turgescence may be accompanied to a considerable extent by thermal contraction.

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* In the thermal contraction of tendons and strings I have not yet been able to convince myself of a decrease in volume.

Moreover, it is doubtful if we have in this case to deal with a phenomenon common to all kinds of muscular fibres. Most probably the mutual relations of the two processes differ with different kinds of muscle.

Hence, we may conclude that chemical contraction by turgescence of the inotagmata is most likely a constant concomitant of the thermal contraction of living muscle, but that compared with the latter, in a single contraction, at least of striated fibres, the former is of little or no consequence as regards the shortening effect.

Chemiotonus and Thermotonus.—Both processes will probably also take part in varying proportion in the *tonus* of muscle, which in some cases will approach more to pure *chemiotonus*, in others more to pure *thermotonus*. Experimental researches in this direction are wanted.

Causes of the Relaxation of Muscle. Theoretical Considerations. Conclusion.—With regard to the *relaxation* of muscle, according to our theory this must be caused either by cooling, or by the withdrawal of water from the doubly-refractive particles. Indeed, we have found that doubly-refractive histological elements in general, even if they be lifeless, like our violin strings, lengthen again upon cooling after they have been contracted by heat, and that they lengthen upon neutralisation or diffusion, after they have been contracted by imbibition at an ordinary temperature.

In a normal relaxation the muscle seems to return completely to its initial state. Of course its store of energy has diminished in proportion to the quantity of mechanical work and heat which has proceeded from it, but, on account of the relatively infinitesimal quantity of substance which is thereby consumed, this return will necessarily seem to be complete even in the case of an isolated muscle.

If we analyse the phenomena of relaxation more exactly, we shall light on several possibilities, the discussion of which would be very interesting with regard to the theory of muscle-life. I shall restrict myself to the phenomena of the relaxation following on *thermal* contraction.

Here, in the first place, we might conceive that the doubly-refractive inotagmata are destroyed in the thermal shortening, so that each of them performs its function once only. The lengthening of the muscular fibrils would then probably be caused solely by the elastic powers of the parts passively extended or compressed by the shortening of the inotagmata. Upon a fresh stimulation, other inotagmata would, in consequence of the combustion of other thermogenic molecules, become active, perish, &c. Through the activity of the formative matter of the living muscle-fibre, the place of the lost inotagmata would be continually or periodically filled by others, probably through the same process of organic crystallisation by which during ontogenesis the doubly-refracting particles in the muscle are produced and arranged.

Against this hypothesis, however, or at least against its general validity, various objections may be put forward. I will mention but two of the most important of them.

There seems to be no doubt but that the doubly-refractive particles of the muscle consist of an albuminous substance, and that they together make up a sensible part of the whole albumin of the muscle-fibrils. In that case it would be most improbable that a great increase of muscular work should not at all, or only very slightly, increase the elimination of nitrogen. To account for this, we should have to recur to an auxiliary hypothesis, and assume either that the nitrogenous remainder of the destroyed inotagma is retained within the body—perhaps in the muscle—for purposes of anabolism, or, which is indeed most improbable, that other organs saved just as much albumin as was decomposed above the normal quantity during the contraction of the muscles.

A second objection consists in the fact that after heating or tetanising muscles until they are rigid, the doubly-refractive power of the sarcous elements will be found still very great.

The other possibility is that the inotagmata may be preserved, and consequently on cooling may return to their former state, and therefore will do work by shortening as often as we choose. In this case muscle would not only seem to offer, but would in fact offer a most striking resemblance to a thermodynamic machine, the solid particles of the framework of which are not destroyed through the chemical process producing the actual energy. No more than such a machine would the muscle require a perpetual renewal of its framework for the continuation of its activity; it would only want a periodic supply of fresh combustible material.

This representation, as you see, will sufficiently account for the fact, which would otherwise remain surprising, that muscular work has such a small influence on the elimination of nitrogen. The facts of microscopic observation also agree with it.

But a further discussion of the two possibilities would lead us too far. The purpose of this lecture was not to give a complete account of all the phenomena of muscular activity. I have wished chiefly to draw your attention to a series of facts which I hold to be of great importance for a deeper insight into the essence of muscular contractility, in so far as they prove the existence of certain material dispositions and processes (admitting of closer experimental examination), by means of which mechanical work may be generated in the muscle by chemical energy.

As with all problems of natural philosophy we must forego the complete solution, and content ourselves with approximations to truth, so now we will refrain from calling out with Archimedes, *εὕρηκα*, but rather be mindful of the words of the apostle:

> *οὐχ ὅτι ἤδη ἔλαβον, διώκω δὲ εἰ καὶ καταλάβω.*

Presents, March 14, 1895.

Transactions.

Belgrade:—Servian Academy. Glas. No. 45. 8vo. *Belgrade* 1894; [and another in Servian.] 8vo. The Academy.

Berlin:—Gesellschaft für Erdkunde. Verhandlungen. Bd. XXII. No. 2. 8vo. *Berlin* 1895; Zeitschrift. Bd. XXIX. No. 6. 8vo. *Berlin* 1894. The Society.

Brussels:—Société Belge de Géologie de Paléontologie et d'Hydrologie. Tomes I—VII. 8vo. *Bruxelles* 1887–94. The Society.

Bucharest:—Societăţe de Sciinţe Fizice. Buletin. Anul III. Nos. 9, 10. 8vo. *Bucurescĭ* 1894. The Society.

Cambridge, Mass.:—Harvard College. Annual Reports of the President and Treasurer. 1893–94. 8vo. *Cambridge* 1895. The College.

Cordova:—Academia Nacional de Ciencias. Boletin. Tomo XIV. Entr. 1. 8vo. *Buenos Aires* 1894. The Academy.

Eastbourne:—Natural History Society. Transactions. Vol. II. Part 8. 8vo. *Eastbourne* [1894]. The Society.

Kew:—Royal Gardens. Bulletin of Miscellaneous Information. February, 1895. 8vo. *London*. The Director.

London:—British Museum (Natural History). A Monograph of the Mycetozoa. 8vo. *London* 1894; Catalogue of the Snakes. Vol. II. 8vo. *London* 1894. The Museum.

Marine Biological Association. Journal. Vol. III. No. 4. 8vo. *London* 1895. The Association.

Royal Photographic Society. Journal and Transactions. Vol. XIX. No. 6. 8vo. *London* 1895. The Society.

Society of Biblical Archæology. Proceedings. Vol. XVII. Part I. 8vo. *London* 1895. The Society.

Manchester:—Geological Society. Transactions. Vol. XXIII. Parts 3, 4. 8vo. *Manchester* 1895. The Society.

Munich:—K. B. Akademie der Wissenschaften. Sitzungsberichte. 1894. Heft 4. 8vo. *München* 1895. The Academy.

Transactions (*continued*).

Paris:—École Normale Supérieure. Annales Scientifiques. Tome XII. No. 2. 4to. *Paris* 1895. The School.

Société Mathématique. Bulletin. Tome XXII. No. 10. 8vo. *Paris* [1894]. The Society.

Prague:—Gesellschaft zur Förderung Deutscher Wissenschaft, Kunst und Literatur in Böhmen. Rechenschafts-Bericht. 1894. 8vo. *Prag* 1895; Bibliothek Deutscher Schriftsteller aus Böhmen. Bd. II. 8vo. *Prag* 1895; Studien zu Euripides; von E. Holzner. 8vo. *Prag* 1895. The Society.

Rome:—R. Comitato Geologico d'Italia. Bollettino. Vol. XXV. No. 4. 8vo. *Roma* 1894. The Committee.

Saint John:—Natural History Society of New Brunswick. Bulletin. Vol. XII. 8vo. *Saint John* 1894. The Society.

Vienna:—Kais. Akademie der Wissenschaften. Denkschriften. Bd. LXI. 4to. *Wien* 1894; Sitzungsberichte (Math.-naturw. Classe). Bd. CIII. Abth. 2a. Heft 8. Abth. 3. Heft 8—10. 8vo. *Wien* 1894. The Academy.

K.K. Geologische Reichsanstalt. Verhandlungen. 1894. Nos. 14—18. 8vo. *Wien.* The Institute.

Observations and Reports.

Calcutta:—Meteorological Department, Government of India. Monthly Weather Review. September, 1894. 4to. *Calcutta;* Meteorological Observations recorded at Seven Stations in India. September, 1894. 4to. [*Calcutta.*] The Department.

Cambridge, Mass.:—Astronomical Observatory of Harvard College. Annals. Vol. XXXII. Part 1. Vol. XLI. No. 2. 4to. *Cambridge* 1895. The Observatory.

Trieste:—Osservatorio Astronomico-Meteorologico. Rapporto Annuale. Vol. IX. 4to. *Trieste* 1895. The Observatory.

Washington:—Nautical Almanac Office. The Elements of the Four Inner Planets and the Fundamental Constants of Astronomy; by Simon Newcomb. 8vo. *Washington* 1895. The Office.

Journals.

American Journal of Philology. Vol. XV. No. 4. 8vo. *Baltimore* 1894. The Editor.

Annaes de Sciencias Naturaes. Anno. II. No. 1. 8vo. *Porto* 1895. The Editor.

Archives des Sciences Biologiques. Tome III. No. 3. 8vo. *St. Pétersbourg* 1894. Institut Imp. de Méd. Exp.

Journals (*continued*).

Horological Journal. Vol. XXXVI. No. 439. 8vo. *London* 1895. British Horological Institute.

Nature Notes. Vol. VI. No. 63. 8vo. *London* 1895. Selborne Society.

Stazioni Sperimentali Agrarie Italiane. Vol. XXVIII. Fasc. 2. 8vo. *Modena* 1895. R. Stazione Agraria, Modena.

Bell (Sir C.), F.R.S. Letters, selected from his Correspondence with his brother, G. J. Bell. 8vo. *London* 1870. Mrs. Francis J. Bell.

Helmholtz (H. von), For. Mem. R.S. Handbuch der Physiologischen Optik. 2te Auflage. Lief. 10. 8vo. *Hamburg* 1895. The Publisher.

Janet (C.) Études sur les Fourmis. [Four excerpts.] 8vo. 1894; Transformation artificielle en Gypse du Calcaire friable des Fossiles des Sables de Bracheux. 8vo. *Paris* 1894. The Author.

Sampson (F. A.) A Bibliography of the Geology of Missouri. 8vo. *Jefferson City* 1890 [and nine other Pamphlets on Missouri.] 8vo. The Author.

Stas (J. S.), For. Mem. R.S. Œuvres Complètes. 3 vols. 4to. *Bruxelles* 1894. Stas Memorial Committee.

March 21, 1895.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

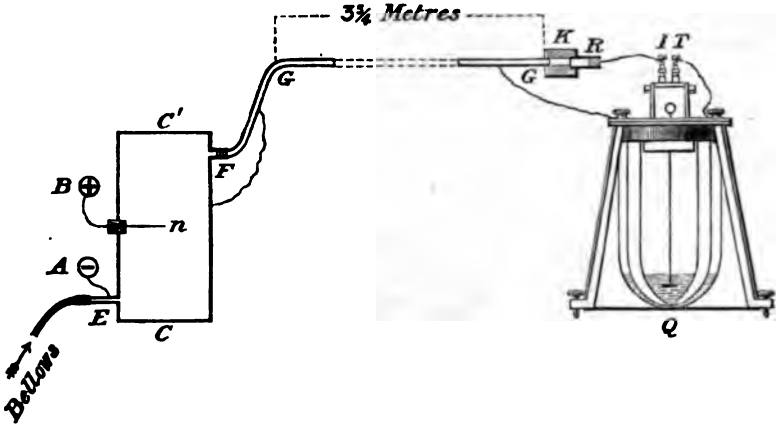
A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Diselectrification of Air." By LORD KELVIN, P.R.S., MAGNUS MACLEAN, M.A., F.R.S.E., and ALEXANDER GALT, B.Sc., F.R.S.E. Received March 14, 1895.

§ 1. The experiment described in § 14 of our paper on the "Electrification of Air and other Gases by bubbling through Water and other Liquids" ('Roy. Soc. Proc.,' February 21, 1895), proves that air, electrified negatively by bubbling through water and caused to pass through a metallic wire gauze strainer, gives up some, but not a large proportion, of its electricity to the metal. We have now made a fresh experimental arrangement for the purpose of investigating diselectrification of air which has been electrified, whether positively or negatively, by other means than bubbling through water: with apparatus represented in figs. 1 and 2, which is simplified from that of our former paper by the omission of the apparatus for electrifica-

FIG. 1.



tion by bubbling, and for collecting large quantities of electrified air.

§ 2. In fig. 1, A B represent the two terminals of a Voss electric machine connected, one of them to a metal can, CC′ (a small biscuit canister of tinned iron), and the other to a fine needle, of which the point *n* is in the centre of the can. The wire making the connection to the needle passes through the centre of a hole in the side of the can, stopped by a paraffin plug. Air is blown from bellows through a pipe, E, near the bottom of the can, and allowed to escape from near the top through an electric filter, F, called the tested filter, from which it passes through a long block-tin pipe, GG, about 3¾ metres long and 1 cm. internal diameter, and thence through a short tunnel in a block of paraffin, K. From this, lastly, it passes through a second electric filter, R, into the open air. This second filter, which we sometimes call the *testing filter*, sometimes the *electric receiver*, is kept in metallic connection with the insulated terminal, I, of a quadrant electrometer, Q. The metal can and the block-tin pipe are metallically connected to the outer case and uninsulated terminal, T, of the quadrant electrometer.

§ 3. The testing filter or electric receiver consists of twelve discs of brass-wire cloth fixed across the mouth of a short metal pipe supported on the end of the paraffin tunnel in the manner represented in fig. 2, on a scale of twice the size of the filter which we have actually used, or of true size for a filter on a tube of 2 cm. diameter, which for some purposes may be better. One of eleven similar discs, of size adapted to a tube of 2 cm. diameter, and an outermost disc with projecting lugs, are shown, true size, and with the gauge of the wire-cloth which we have actually used, shown true size, in fig. 3. The eleven little circular discs of wire cloth are held in position by bending over them the four lugs belonging to the outermost disc, and all are kept compactly together by a short piece of india-rubber tube stretched over them outside as shown in fig. 2.

§ 4. We commenced with a few experiments to test the efficiency of the testing filter, R, with no tested filter at F, and merely continuous block-tin pipe, FGG, from the can to the paraffin tunnel. First, working the bellows with no electrification of the needle point, we found no sensible electric effect on the electrometer, which proved that, whether from natural electrification of the air of the laboratory, or by the action of the bellows, or by the passage of the air through the long metal pipe, no electrification sensible to our test was produced. After that we kept the needle point, *n*, electrified, either positively or negatively, for five or six minutes at a time by turning the little Voss machine, and we found large effects rising to about 3½ volts in five minutes, positive or negative, according as *n* was positive or negative.

Fig. 2.

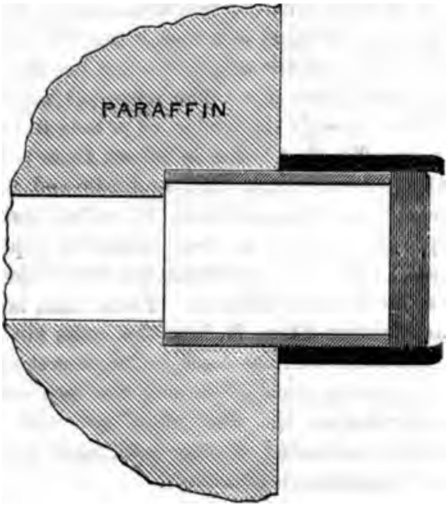
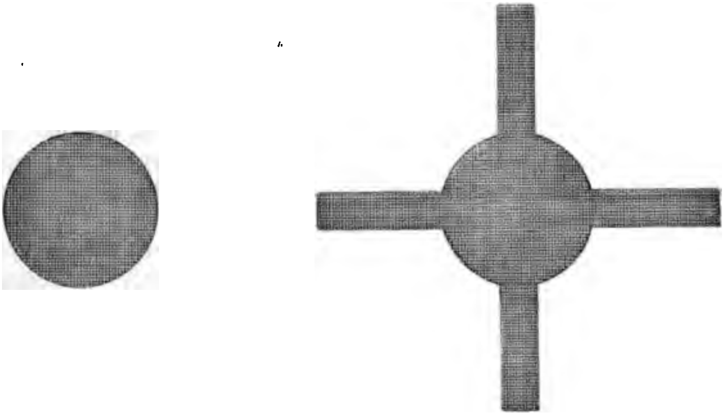


Fig. 3.

Twenty-five wires to the centimetre. Diameter of each wire is 0·16 mm. Hence each aperture is 0·24 mm. square.



§ 5. The apparatus is now ready to test the efficacy of filters or other appliances of different kinds placed at F for the purpose of diselectrifying air which has been electrified, whether positively or negatively, by the electrified needle point *n*. We began with a filter of 12 wire-gauze discs, placed at F and kept in metallic connection with the tin pipe outside. This nearly halved the electricity shown by the electrometer. We then tried 24, 48, 72, 96, 120 wire-gauze discs, successively, placed in groups of 24, and separated from one another by short lengths of 2 cm. of lead tube, in the line of the flow of the air between F and G (fig. 1), all kept in metallic connection with the block-tin pipe and the outer case of the electrometer. We were surprised with the smallness of the additions to the diselectrifying efficiency of the 12 strainers first tried; for example, the filter of 120 wire gauzes only reduced the electrical indication to a little less than one-half of what it was with the 12 which we first tried.

We found that cotton-wool between the spaces in the groups of 24 wire gauzes largely increased the diselectrifying effect. Thus, with 72 wire gauzes and cotton-wool we succeeded in reducing the electrical effect to about one-twelfth of what it was with only a filter of 12 wire gauzes; but hitherto we have not succeeded in rendering imperceptibly small the electricity yielded by the outflowing air to the testing filter R in our method of observation.

§ 6. We intend trying various methods of obtaining more and more nearly complete diselectrification of the electrified air flowing out of the can at F; and this for air electrified otherwise than by the needle point, as shown in the diagram: for instance, by an electrified flame in place of the needle point; or again by bubbling through water or other liquids. Meantime, the mere fact that the electricity, whether positive or negative, given to air by an electrified needle point, can be conveyed through 3 or 4 metres of small metal tube (1 cm. diameter), and shown on a quadrant electrometer by a receiving filter, is not without interest. We may add now that, with the receiving filter removed and merely a fine platinum wire put in the mouth of the paraffin tunnel, we have found that enough of electricity is taken from the outflowing air to be amply shown by the quadrant electrometer; which renders even more surprising the fact that the diselectrifying power of 120 strainers of fine wire-gauze should be so small as we have found it.

II. "On the Conditions affecting Bacterial Life in Thames Water." By E. FRANKLAND, D.C.L., F.R.S. Received January 31, 1895.

Since May, 1892, I have been making monthly determinations of the number of bacteria capable of development on a peptone-gelatine plate in a given volume of Thames water collected at the intakes of the Metropolitan water companies at Hampton. The number of microbes per cubic centimetre of water varied during this time between 631 and 56,630, the highest numbers having, as a rule, been found in winter or when the temperature of the water was low, and the lowest in summer or when the temperature was high.

Amongst the conditions which favour or retard the development of microbial life in river water, temperature, rainfall, and sunshine or gloom are probably the most important, sunshine having been recently shown by Dr. Marshall Ward to be, under certain circumstances, extremely potent in the destruction of bacteria. The following tables contain the results of these microbe determinations placed in juxtaposition with (A) the temperature of the water at the time the sample was taken, (B) the number of hours of sunshine on the day and up to the hour when the sample was drawn and on the two preceding days, and (C) the flow of the Thames over Teddington Weir on the same day expressed in millions of gallons per 24 hours.

The samples for microbe cultivation were collected at about 9 in. below the surface of the water in partially exhausted and sealed tubes, the ends of which, when the tubes were lowered to the desired depth, were broken off by an ingenious contrivance devised by my assistant, Mr. W. T. Burgess. On being withdrawn from the river, the tubes were immediately hermetically sealed and packed in ice for conveyance to my laboratory, where the cultivation was always commenced within four hours of the time of collection.

For the records of sunshine I am indebted to the kindness of Professor E. J. Stone, M.A., F.R.S., the Radcliffe Observer at Oxford, and to Mr. James B. Jordan, of Staines. Finding that the Oxford observations differed but little from those at Staines, and as Staines is nearer to, although higher up, the river than the place where my samples were collected, I have used the Staines records in the table, except on a few dates when Mr. Jordan's observations had been intermitted.

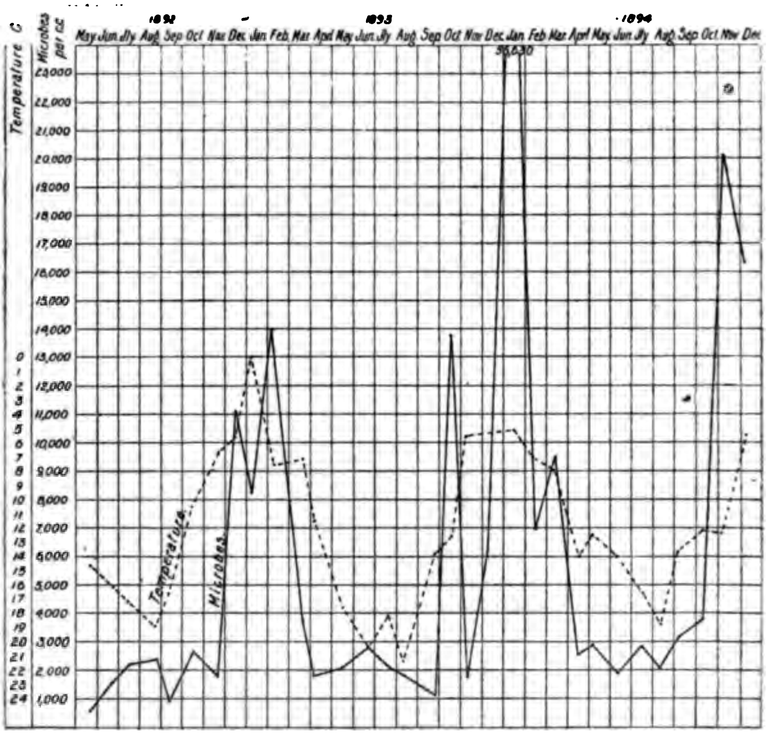
The gaugings of the Thames at Teddington Weir were kindly furnished by Mr. C. J. More, the engineer to the Thames Conservancy Board.

Without the data giving the conditions of sunshine and flow of the river, it was impossible to draw any trustworthy conclusion as to the cause of the increase and diminution of the number of microbes per cubic centimetre of Thames water at Hampton, because the conditions supposed to be favourable for microbial life all, approximately, attain the greatest intensity at the same season of the year; thus, whilst the temperature is lowest in winter, so also, as a rule, are the minimum amount of sunshine and the maximum amount of flood water. The following collateral observations, however, afford definite evidence as to which of the three conditions—temperature, sunshine, and flow of the river—has the predominant influence upon bacterial life in the water. The first table and diagram compare the

Table A.—Comparison of Number of Microbes with the Temperature of Thames Water.

Date.	Temperature, C.	Number of microbes per c.c.	Date.	Temperature, C.	Number of microbes per c.c.
1892.			1893.		
May 20	14°·8	631	September 25	13°·8	1,158
June 20	16·2	1,658	October 19	12·5	13,790
July 13	17·2	2,268	November 9	5·6	1,789
August 25	19·1	2,421	December 7	5·4	6,316
September 12	16·6	947			
October 17	10·5	2,316			
November 22	6·7	1,868	1894.		
December 15	5·6	11,158			
			January 15	5·2	56,630
			February 13	6·9	6,947
1893.			March 13	8·0	9,480
			April 13	14·0	2,520
January 9	0	8,210	May 3	12·5	2,880
February 4	7·6	13,947	June 11	14·1	1,840
March 20	7·2	3,737	July 16	16·7	2,860
April 5	11·6	1,763	August 11	18·7	2,080
May 16	17·6	2,052	September 7	13·8	3,080
June 20	20·4	2,789	October 10	12·2	3,800
July 19	18·2	2,132	November 5	12·3	20,080
August 10	21·3	1,895	December 10	5·5	16,300

DIAGRAM No. 1.



number of microbes per cubic centimetre with the temperature of the water at the time the samples were taken. In this diagram the ordinates express the numbers of microbes and the temperatures, whilst the abscissæ denote the months when the samples were taken. For obvious reasons the ordinates expressing numbers of microbes and temperature are numbered in opposite directions.

From this table and diagram, high temperature would appear to have been unfavourable to microbes in May, June, July, August, and September, 1892, when the number per cubic centimetre was small; but in October and November the number still remained small, although the temperature in these months was much lower. In December, however, of the same year the temperature remained much the same as in November, but the microbes underwent an enormous increase. Again, in the following year (1893), the temperature in January on the day when the sample was collected was still lower, being at the freezing point, but the microbes were considerably less numerous. On the other hand, in February, when the temperature was higher, the microbes were much more numerous than even in the previous December. In March, whilst the temperature remained practically the same as in February, the number of microbes per cubic centimetre fell from about 14,000 to about 3,700. Again, in April, whilst the temperature was still moderate (11·6° C.), the number of microbes fell still further to less than 2,000 per cubic centimetre. In May, June, July, and August, the temperature was high and the number of microbes uniformly small, but in September there was a great reduction of temperature, but accompanied also by a considerable diminution in the number of microbes. In October there was again, as in February, an enormous development of bacteria whilst the temperature was only slightly lower than in the previous month, when the number was remarkably small. In November again, with a much lower temperature, there was a reduction in the number of microbes from about 14,000 in the previous month to about 1,750 per cubic centimetre. During the following months of November and December, 1893, and January, February, and March, 1894, whilst the temperature remained low and nearly constant, the microbes were sometimes as low as 1,789, and sometimes as high as 56,630; but, during the following months—April, May, June, July, August, September, and October—the increase of temperature and diminution of microbes, and *vice versâ*, marched very much *pari passu*. In November, however, with no alteration of temperature as compared with the previous month, the number of microbes increased from 3,800 to 20,080 per cubic centimetre, whilst in December there was a diminution alike in temperature and microbes.

Thus it is evident that, although coincidences between a high number of microbes and a low temperature are not wanting, some other condition entirely masks the effect of temperature.

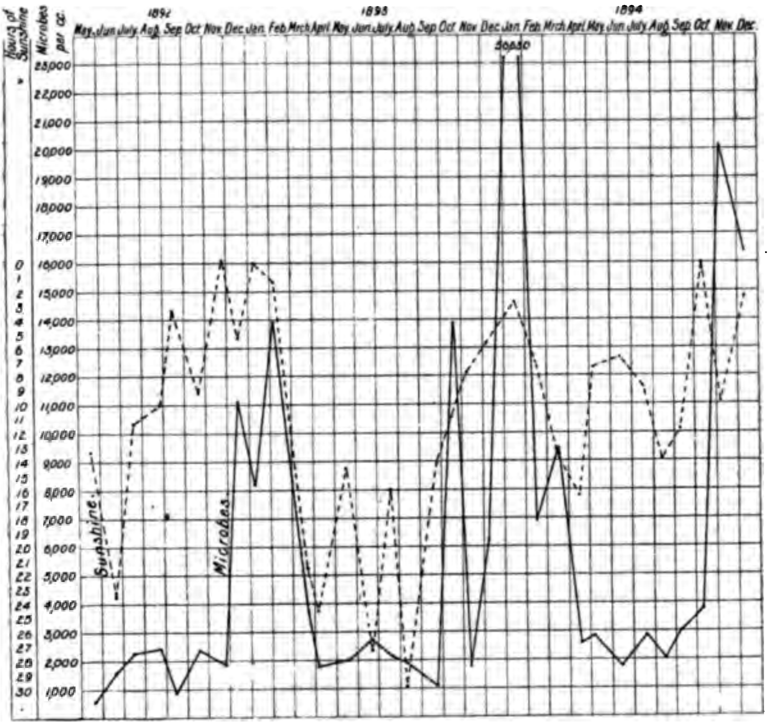
The next table and diagram institute a comparison between the number of microbes and the hours of sunshine to which the water has been exposed. The diagram is constructed on the same lines as No. 1.

Reference to the table and diagram shows that, whilst during the months May to November, 1892, there was an enormous variation in the amount of sunshine, namely, from 23½ hours in June to none in November, there was practically no corresponding variation in the number of microbes. In December of that year, there was much more sunshine than in the previous month, but the microbes, instead of being diminished thereby, increased from less than 2,000 to over 11,000 per cubic centimetre; and, although there was no sunshine on the three days previous to the taking of the sample in January, 1893, the number of microbes actually decreased about 3,000 per cubic centimetre. In the following month of February, however, about 14,000 microbes per cubic centimetre coincided with but little

Table B.—Comparison of Number of Microbes with Hours of Sunshine.

Date.	Hours of sunshine during 3 days at Staines.	Number of microbes per c.c.	Date.	Hours of sunshine during 3 days at Staines.	Number of microbes per c.c.
1892.			1893.		
	h. m.			h. m.	
May 20	13 10	631	September 25	14 5	1,158
June 20	23 30	1,658	October 19	11 5	13,790
July 13	11 10	2,268	November 9	7 45	1,789
August 25	9 55	2,421	December 7	5 45	6,316
September 12	3 15	947			
October 17	9 15	2,316			
November 22	0	1,868	1894.		
December 15	5 20	11,158			
			January 15	2 45	56,630
			February 13	7 0	6,947
1893.			March 13	13 40	9,480
			April 13	16 20	2,520
January 9	0	8,210	May 3	7 30	2,880
February 4	1 20	13,947	June 11	6 35	1,840
March 20	21 30	3,737	July 16	8 45	2,860
April 5	24 30	1,763	August 11	13 55	2,080
May 16	14 30	2,052	September 7	11 50	3,080
June 20	27 25	2,789	October 10	0	3,800
July 19	15 55	2,132	November 5	9 50	20,080
August 10	29 55	1,895	December 10	2 15	16,300

DIAGRAM No. 2.



more than an hour of sunshine. In the following month of March again, an enormous increase of sunshine was followed by a corresponding decrease of microbes, and this relation was continued in the following month; but in May, with a great diminution of sunlight, there was practically no increase of microbes, and in June, with an enormous increase of sunlight, there was the anomaly of an increase, though slight, of bacteria.

In the following July and August, there were enormous alternations of sunshine and gloom with no corresponding difference in the number of microbes; whilst in September, with a great diminution in sunshine, there was observed one of the smallest numbers of microbes recorded. In the following month of October, however, a small diminution of sunshine was accompanied by a very large increase of microbes. On the other hand, in the month of November, with a still smaller amount of sunshine there was an enormous reduction in the number of microbes.

In January, 1894, a small amount of sunshine was followed by an enormous number (56,630) of microbes. In the following month of February, however, this number was reduced to about 7,000, although the amount of sunshine was not very much greater. In the following month of March, there was a great increase both of sunshine and bacteria, whilst in April, there was an increase of sunshine and a great diminution in the number of microbes. In May, however, there was a great decrease of sunshine, but a very slight increase of microbes. In June there was rather less sunshine than in the previous month, but also fewer microbes; whilst in July, an increase of sunlight was accompaned by an increase of microbes. In August, September, and October, with each diminution of sunshine there was a corresponding increase of microbes; but in November, with a very moderate amount of sunlight, there was an enormous increase of microbes from 3,800 to 20,080 per cubic centimetre, whilst in the following month of December a considerable diminution of sunlight was found to be compatible with a marked decrease of microbes.

Thus it is evident that, as in the case of temperature, there is some other condition which entirely overbears the influence of sunlight in the destruction of microbes in the river water. This condition is the amount of rainfall higher up the river, or, in other words, the volume of water flowing along the river bed, as is seen from the comparison presented in the following table and diagram (pp. 447, 448).

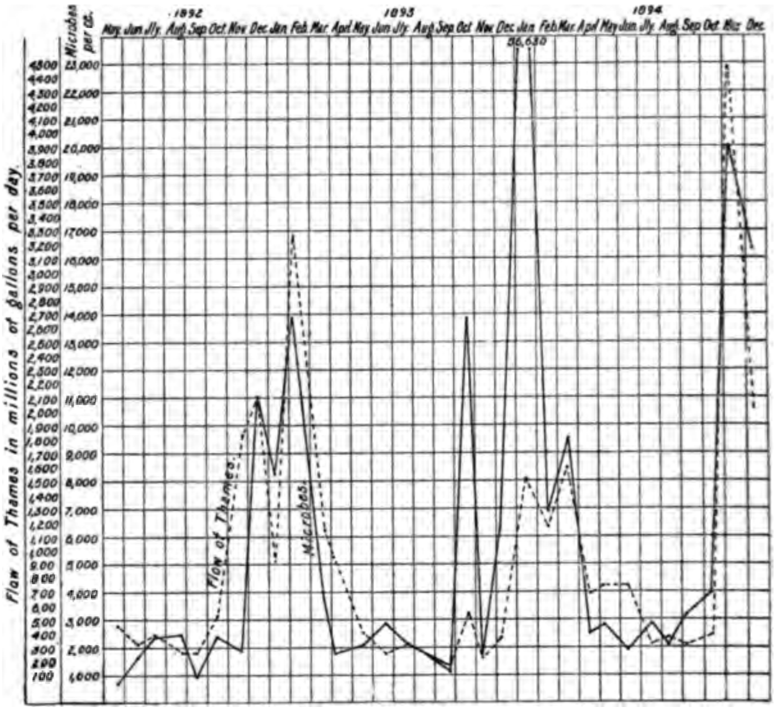
This table and diagram show very conclusively that the volume of water flowing in the Thames is the paramount influence determining the number of microbes in the water. They compare the volume of water in the river, as gauged at Teddington Weir, with the number of microbes found in the raw Thames water at Hampton on the same day. In the diagram, the numbers representing the flow of the river in millions of gallons and the number of microbes per cubic centimetre of water both run from the bottom of the diagram upwards.

Comparing the numbers in the table and the curves on the diagram, it is seen that, with a few exceptions, a remarkably close relation is maintained between these numbers and curves respectively; thus during the months of May, June, July, August, September, and October, 1892, the river was low and the number of microbes small. In December of the same year, the daily flow of the river had risen to 2,105 millons of gallons, and the microbes to 11,158 per cubic centimetre; whilst in January 1893, the flow had decreased to 915 millions of gallons, and the microbes to 8,210 per cubic centimetre. In February of the same year, the flow of water over Teddington Weir had risen to 3,255 millions of gallons, and the number of microbes simultaneously to 13,947 per cubic centimetre; whilst in March, with a reduced flow of 1,175 millions of gallons, the number of microbes came down to 3,737 per cubic centimetre, and this was followed in April by a further diminution to 1,763 per cubic centimetre, whilst

Table C.—Comparison of Number of Microbes with Rate of Flow of River on same Day.

Date.	Daily flow of river.	No. of microbes per c.c.
1892.	Gallons.	
May 20	480,000,000	631
June 20	335,000,000	1,658
July 13	385,200,000	2,268
August 25	276,300,000	2,421
September 12	270,000,000	947
October 17	501,300,000	2,316
November 22	1,845,000,000	1,868
December 15	2,105,000,000	11,158
1893.		
January 9	915,000,000	8,210
February 4	3,255,000,000	13,947
March 20	1,175,000,000	3,737
April 5	985,000,000	1,763
May 16	420,000,000	2,052
June 20	277,200,000	2,789
July 19	333,000,000	2,132
August 10	264,600,000	1,895
1893.	Gallons.	
September 25	159,300,000	1,158
October 19	542,700,000	13,790
November 9	249,300,000	1,789
December 7	375,000,000	6,316
1894.		
January 15	1,510,000,000	56,630
February 13	1,140,000,000	6,947
March 13	1,600,000,000	9,480
April 13	690,000,000	2,520
May 3	740,700,000	2,880
June 11	745,200,000	1,840
July 16	369,900,000	2,860
August 11	360,900,000	2,080
September 7	336,600,000	3,080
October 10	396,000,000	3,800
November 5	4,462,200,000	20,080
December 10	2,058,300,000	16,300

DIAGRAM No. 3.



the flow of the river was simultaneously reduced to 985 millions of gallons daily.

In the following months, May, June, July, August, and September, the river remained low, and so did the number of microbes; but in October, with an increase of flow from 159 to 543 millions of gallons, the number of microbes increased from 1,158 to 13,790 per cubic centimetre; whilst, in the following month of November, the flow of the river was reduced to 249 millions of gallons and the microbes to 1,789 per cubic centimetre. Again, in the following month of December, the flow of the river increased to 375 millions of gallons, and the microbes to 6,316 per cubic centimetre.

In January, 1894, the flow of the river was augmented to 1,510 millions of gallons, and the microbes to the enormous number of 56,630 per cubic centimetre. In the following months of February, March, April, and May, the two curves follow each other with remarkable regularity; whilst in July, August, September, and October, both the flow of the river and the number of microbes remained low, there being a slight increase in both from September to October.

Then came the tremendous flood of November, the highest on record, the flow of the river over Teddington Weir, on the 18th, having reached nearly 20,136 millions of gallons. The sample for microbe cultivation was, however, taken on the 5th, when the flow only amounted to 4,462 millions of gallons per day, but this increase from 396 millions of gallons in October to 4,462 millions on the 5th of November was accompanied by an increase in the number of microbes from 3,800 on the 10th of October to 20,080 per cubic centimetre on the 5th of November; whilst, in December, the flow of the river had fallen to 2,058 millions of gallons per day, and the number of microbes to 16,300 per cubic centimetre.

The only exception of any importance to the rule, that the number of microbes varies with the flow of the river, occurring during the thirty-two months through which these observations have been continued, happened in November, 1892, when the flow increased from 501 millions of gallons in October to 1,845 millions in November, whilst the microbes actually diminished in number from 2,316 to 1,868 per cubic centimetre. Neither the sunshine nor the temperature records of these two months, however, afford any explanation of this anomalous result, for there was a good deal of sunshine in October before the collection of the sample, and the temperature was higher; whilst in November, no ray of sunshine reached the Thames during the three days preceding the taking of the sample, and the temperature was nearly 4° C. lower than in the preceding month.

These comparisons, therefore, demonstrate, I think satisfactorily, that the number of microbes in Thames water depends upon the rate of flow of the river or, in other words, upon the rainfall, and but slightly, if at all, upon either the presence or absence of sunshine or a high or low temperature.

With regard to the effect of sunshine upon bacterial life, the interesting researches of Dr. Marshall Ward leave no doubt that sunlight is a powerful germicide; but it is probable that its potency, in this respect, is greatly diminished if not entirely annulled, when the solar rays have to pass through a stratum of water even of comparatively small thickness before they reach the living organisms. If this be the case, it can be no matter for surprise that the effect of sunshine upon bacterial life in the great mass of Thames water should be nearly, if not quite, imperceptible.

Note added March 16.

Since this paper was written, I have ascertained that between October 17 and November 22, 1892, when the sample for microbe cultivation was collected, the river had several times been in such high flood as to be bank-full. Thus, between October 30 and

November 2, the flow was never less than 3,000 millions of gallons per day, and on November 2 it reached 4,240 millions. Again, on the 17th the flow was 3,305 millions, and on the 18th, 4,165 millions. It then gradually decreased to 1,845 millions on the day when the sample was drawn. Thus the Thames basin had been twice very thoroughly washed out immediately before the time when the November sample was taken. There had previously been no such floods after the 5th of January in that year. This condition of things affords a fairly satisfactory explanation of the anomalous result yielded by this sample.

III. "The Cause of Luminosity in the Flames of Hydrocarbon Gases." By Vivian B. Lewes, Professor of Chemistry at the Royal Naval College, Greenwich. Communicated by Professor Thorpe, F.R.S. Received February 14, 1895.

In a paper read before the Chemical Society in 1893, I showed that in the inner non-luminous zone of a flame of ordinary illuminating gas, the hydrocarbons originally present in the gas, and consisting of ethylene, butylene, benzene, methane, and ethane, became converted by the baking action of the walls of flame between which they had to pass into acetylene, and that at the moment when luminosity commenced, over 80 per cent. of the total unsaturated hydrocarbons present consisted of this compound.

The presence of acetylene at the point where luminosity commenced naturally suggested that it was in some way due to actions in which the acetylene played the principal part—either that it split up into carbon and hydrogen under the influence of heat, and so supplied the flame with the solid particles necessary, according to Sir Humphry Davy's theory of the cause of luminosity, or else by its polymerisation it formed the dense vapours required by Dr. E. Frankland's more recent hypothesis.

In order to elucidate this point, I carried out the long series of experiments upon the action of heat upon flowing ethylene and other hydrocarbons, which formed the subject of communications to the Royal Society in 1893 and early this year, in which I showed that whilst flowing through a heated area (the temperature of which was between 800° and 1000° C.), ethylene decomposed according to the equation

$$3C_2H_4 = 2C_2H_2 + 2CH_4,$$

and that the acetylene then polymerised into a large number of more complex hydrocarbons, amongst which benzene and naphthalene were conspicuous, whilst at temperatures above 1200° C., no polymerisation took place, but the acetylene formed from the ethylene decomposed at once into carbon and hydrogen, whilst the methane, which up to this temperature had been but little affected, decomposed into

$$2CH_4 = C_2H_2 + H_2,$$

and this fresh supply of acetylene at once broke up to carbon and hydrogen, so that at temperatures above 1200° C. the complete action might be looked upon as being

$$C_2H_4 = C_2 + 2H_2.$$

These results have an important bearing upon the cause of the luminosity in the flame, as it is manifest that if the temperature of the luminous zone is above 1200° C., the light emitted must be due to incandescent particles of carbon, and not to incandescent hydrocarbon vapours.

On determining the temperature of an ethylene flame whilst burning from a small fish-tail burner by means of the Le Chatelier thermocouple, used in the way described in my paper* on the luminosity of coal-gas flames, I found that the temperatures were as follows:—

Portion of flame.	Height above burner.	Temperature.
Non-luminous zone	½ inch.	952° C.
Commencement of luminosity ...	1¼ „	1340
Top of luminous zone	2 „	1865
Sides of „ „		1875

showing that luminosity commenced at 1340° C., and continued even at 1875° C., temperatures at which the incandescent vapour theory becomes untenable.

It might be urged that the heavy hydrocarbons already produced at a lower temperature in the non-luminous zone are not so easily decomposed by heat as acetylene, and that these may be causing the luminosity, even though carbon particles be present from the decomposed acetylene; but this would hardly be possible, as so little besides acetylene is to be found at the top of the non-luminous zone of an ethylene flame, and it can be experimentally shown that even when benzene vapour is formed and is largely diluted it begins to break up and deposit carbon at 1200° C.

The supporters of the "solid particle" theory of luminosity agree in concluding that the liberated carbon, existing as it does in a condition of molecular division, is heated to incandescence partly by its own combustion, and partly by the combustion of the hydrogen and carbon monoxide going on around the finely-divided carbon particles.

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* 'Chem. Soc. Journal,' 1893.

As has been pointed out by many observers, it is clear that the carbon particles themselves undergo combustion, otherwise they would escape unburnt from the flame, whilst it is manifest that the combustion of hydrogen and carbon monoxide, which plays so important a part in the flame, must add its iota to the temperature attained by them.

Both these sources of temperature, however, would be manifest in the flame itself, and with flames of given size burning from the same description of jet we ought to find that their luminosity is governed by—

A. The temperature of the flame.
B. The number of carbon particles in a given area.

Moreover, we should expect that the higher the temperature of the flame, the whiter would be the light emitted, so that a comparatively low temperature flame, even when rich in carbon particles, would be yellow and lurid as compared with a flame containing the same or a smaller number of particles, but which had a higher temperature.

It has been pointed out by Professor A. Smithells* that it is erroneous to consider the temperature of a flame as being the temperature recorded by thermometric instruments inserted into the flame, as by such devices you only obtain the mean temperature of a considerable area of the flame uncorrected for loss from conduction.

It is also perfectly well known that in a flame a thick platinum wire may only be heated to redness, whilst a thin wire may even be fused, and this suggests that flame temperatures taken by the Le Chatelier thermo-couple of platinum and platinum-rhodium wires may be totally incorrect. In using this beautiful and convenient device, I have found that the length of the wires twisted together made practically no difference in the recorded temperature, and that one twist was as good as six.

In all my flame experiments I have made the twist as short as possible, and by always using wires of the same thickness have obtained results which are at any rate comparable if not correct, and in order to find what difference the thickness of the wires would make, I got Messrs. Johnson and Matthey to draw for me wires of 0·018, 0·011, and 0·003 of an inch diameter, and having calibrated the galvanometer scale for temperature with thermo-couples of the same length of twist made from each of them, obtained the following results with the same portion of a Bunsen flame.

Wire used.	Temperature shown.
0·018	1617° C.
0·011	1728
0·003	1865

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* 'Phil. Mag.,' 1894, p. 249.

These results show that the diameter of the wire seriously affects the temperature recorded under these conditions by the thermo-couple, the same degree of heat being recorded by the fine wire as being 248° hotter than is shown by the thickest wire employed, this discrepancy being probably chiefly due to loss by conduction.

In taking the temperature of heated gas flowing through a tube this source of error is but small, as some considerable length of wire being heated on each side of the twist, conduction has but little effect on the thermo-couple itself, but in determining the temperature of flames it is manifest that the finest usable wire must be employed in order to reduce the error from conduction. Test experiments also showed that no part of the thermo-couple must project beyond the flame, as if it did a considerable diminution in the recorded temperature took place.

For these reasons it was manifestly best to use the finest wire which could be employed without the risk of fusing at the temperatures existing in the flames to be tested; and all temperatures recorded in this paper were made with wire 0·011 in. in diameter, the twist being as short as possible, so that it is probable that, although the temperatures may be from 100° to 200° too low, yet the results are strictly comparable.

Experiments which I have lately made with pure acetylene, prepared by the action of water upon calcic carbide, show it to be the most powerful illuminant to be found amongst the gaseous hydrocarbons, as when burnt in a small flat flame burner under the most suitable pressure, and its illuminating power calculated to a flow of 5 cubic ft. an hour, its value is equal to about 240 candles.

The colour of the flame is pure white, and an ethylene flame beside it looks yellow and dull—the purity of the light at once suggesting a very high condition of incandescence in the particles of carbon present in the flame.

On now taking the temperature of the various portions of the flame, and comparing these with the temperatures obtained in the same way with the ethylene flame and a coal-gas flame of the same size, the following results are obtained:—

Portion of flame.	Acetylene.	Ethylene.	Coal gas.
Non-luminous zone	459° C.	952° C.	1023° C.
Commencement of luminosity..	1411	1340	1658
Near top of luminous zone....	1517	1865	2116

whilst the illuminating values of the gases calculated to a flow of 5 cubic ft. an hour in the burners best suited for their consumption, are

Acetylene	240·0
Ethylene	68·5
Coal gas......................	16·8

whilst if all were compared when burning from flat-flame burners of the same size as those in which the temperatures were determined, the results when calculated to a consumption of 5 cubic ft. an hour would be

Acetylene	211·0
Ethylene......................	31·5
Coal gas......................	nil

Here then we have the anomaly of three gases, which not only do not conform to the preconceived expectation, but which have their ratio of temperature and illuminating value directly opposed to each other.

In the case of the acetylene and ethylene, moreover, the molecules contain the same number of atoms of carbon, and yet we obtain so enormous a discrepancy in their illuminating value.

The fact that there is no apparent relation existing between the temperature of the flame, or the probable number of carbon particles contained in it and its illuminating value, at once suggests that the luminosity must be in great part governed by some thermo-chemical changes taking place in the flame itself, and which do not of necessity affect the average temperature of the flame to any great degree.

The researches of Hittorf* and Siemens show that air, steam, and the oxides of carbon, even when heated to temperatures above those existing in luminous hydrocarbon flames, are perfectly non-luminous, and the fact that the Bunsen flame, when supplied with sufficient air, has a temperature exceeding 1800° C. in its hottest part, and yet emits no light, shows us that it is exceedingly unlikely that any interactions leading to luminosity take place amongst these ordinary flame gases.

The fact that most of the unsaturated hydrocarbons in the flame are converted into acetylene before luminosity commences, naturally draws one's attention to this body, and the fact that it is highly endothermic, at once suggests the idea that it may be the liberation of heat during its decomposition that endows the carbon particles produced from it with an incandescence far higher than any which could be expected from the temperature of the flame.

Berthelot has calculated that the temperature developed by the detonation of acetylene at constant volume is no less than 6220° C., and if this be imparted at the moment of its liberation to the products of its decomposition, the incandescence of the carbon particles is at once explained.

If luminosity be even partly due to this cause, the detonation of pure acetylene first recorded by Berthelot should develop light. In order to see if this were so, a thin glass tube, closed by a cork, had a detonator containing one-tenth of a gram of mercuric fulminate suspended in it by two copper wires, which were connected by a thin platinum wire in contact with the fulminate, and on firing the detonator by the electric current the flash of the fulminate was found to emit but a feeble light.

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* 'Wied. Ann.,' vol. 7, pp. 587, 591.

The same charge was fixed in a similar tube filled with pure acetylene collected over mercury, the result being a flash of intense white light and the shattering of the tube, the pieces of which were thickly coated with the carbon produced by the decomposition of the acetylene.

Moreover, the small piece of white tissue paper used to contain the fulminate was only scorched at the points where the explosion of the fulminate had burst through it, showing that in the instantaneous decomposition which had taken place, the intense heat which had been developed either was confined to the products of decomposition, or else had not had time to scorch the paper.

The experiment at first sight seemed conclusive evidence that it was the endothermic nature of the acetylene which, during its decomposition in the flame, endowed the particles of carbon with the necessary incandescence, but the objection presented itself that, when exploding mixtures of oxygen and hydrogen in the eudiometer, a distinctly luminous flash is produced, and, although the light so obtained is feeble as compared with the intensity of the white light produced by the detonation of the acetylene, still further proof is necessary before this action can be accepted as the prime factor in producing luminosity.

It is also manifest that it would not do to assume that the rapidity of the decomposition of the acetylene in a flame was nearly so great as when the undiluted gas was detonated, and the question arose as to whether it would be possible to obtain evidence as to acetylene, when exposed to heat alone, liberating carbon in a luminous condition.

Although the instantaneous liberation of heat on the decomposition of the gas by detonation appears to confine the temperature to the products of its decomposition, it was to be expected that, on being decomposed by heat, and probably, therefore, at a slower rate, the increase in temperature might be detected.

To try this, pure acetylene was passed through a platinum tube 2 mm. in diameter and 40 cm. long, in which the Le Chatelier thermo-couple was arranged as follows:—The two wires were twisted together for a length of 3 mm., and the wires on either side of the twist are then passed through thin glass tubes, which are fused on to them; having been in this way coated with glass so that only the twist is exposed, they are passed through the platinum tube, the glass insulating the wire from the metal of the tube, and also keeping the thermo-junction in such a position that it registers the temperatures of the gas in the tube, not that of the wall of the tube. To each end of the platinum tube glass T-pieces are fitted, down the stems of which the wires pass to mercury seals; from the metal seals conducting wires lead to the resistance coils, the key, and a reflecting galvanometer.

A steady flow of acetylene was allowed to pass through the tube, and was led into water at the other end. The tube was slowly and carefully heated for about 4 in. of its length, and, as the temperature reached 700° C., white vapours began to flow from the tube, and these, as the temperature rose, increased in quantity. The source of heat had been so regulated that the temperature had risen about 10° per minute, but, almost immediately 800° C. was passed, the galvanometer registered a sudden leap up in temperature to about 1000° C., whilst finely-divided carbon poured from the tube. This seemed to indicate that 800° was about the temperature at which the pure acetylene broke up into its constituents, and an experiment was now made to see if this developed incandescence in the liberated carbon.

A small glass combustion tube was well supported, and heated to the highest temperature attainable with one of Fletcher's big blowpipes, whilst pure acetylene was slowly flowing through it, the heating not being commenced until the tube was filled with the pure gas, all air being thoroughly rinsed out. As the temperature reached the softening point of the glass, the acetylene apparently burst into a lurid flame at the point where it entered the zone of heat, and clouds of carbon swept forwards through the tube; but, although the carbon particles had to traverse an inch or more of tube more highly heated than the point of entering the hot zone, it was only at this latter point that the luminosity was developed, proving beyond doubt that it was the heat evolved by the decomposition, and not the external heating, which caused the carbon particles to emit light.

If it is the decomposition of the molecule of acetylene which develops the heat which is the cause of the incandescence of the carbon particles, then, if acetylene could be burnt without decomposition, a non-luminous flame should be produced. It is conceivable that this might be done by so diluting the acetylene that it would require a much higher temperature to break it up.

It was Heumann who showed* that hydrocarbon gases may burn with luminous flames, *i.e.*, with separation of carbon in the flame, or with non-luminous flames, *i.e.*, without any separation of carbon, and that the maintenance of a high temperature is an essential condition of luminosity: a flame, the temperature of which has been lowered by any means, being no longer able to bring about the required separation of carbon. He also points out* that "combustible matter, when diluted with indifferent gases, requires to be maintained at a higher temperature, in order that it may burn with a luminous flame, than when it is undiluted with such gases."

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* 'Liebig's Annalen,' vol. 183, Part I, pp. 102—131.

Dr. Percy Frankland, in his researches on the effect of diluents upon the illuminating value of hydrocarbons,† showed that ethylene, which was capable of developing a light of 68·5 candles power when burnt by itself, became non-luminous when diluted with about:—

Hydrogen	90 per cent.
Carbon monoxide	80 „
Carbon dioxide	60 „
Nitrogen	87 „

results which all show that excessive dilution by inert gases destroys luminosity.

In order to see if dilution had the same effect upon acetylene, experiments were made by diluting it with pure hydrogen. The gases were mixed over water, the proportion of acetylene actually present in the gas being determined by analysis at the burner, and although the water in both holder and meter was, as far as possible, saturated with the gas, yet, as the analyses show, the precaution was an important one.

Composition of mixture.				Illuminating value of mixture per 5 c.c. when burnt in 00 Bray.
Made in holder.		At burner.		
Hydrogen.	Acetylene.	Hydrogen.	Acetylene.	
90	10	90·5	9·5	nil
80	20	81·5	18·5	1·8
70	30	65·5	34·5	14·0
50	50	43·5	56·5	87·0

Showing that dilution with between 80 and 90 per cent. of hydrogen rendered the acetylene non-luminous when the mixture was burnt from a burner suitable for the higher values of gas.

In order to determine the point at which luminosity was destroyed when consuming the mixture in a burner suited to develop the light from a gas of low illuminating power, the experiment was repeated, using a 3-in. flame burning from the London argand, and also from a No. 4 Bray union jet, the latter being employed as it is difficult to determine the temperature in the argand flame.

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* 'Liebig's Annalen,' vol. 183, Part I, pp. 102—131.

† 'Chem. Soc. Jour.,' vol. 45, p. 30 and p. 227.

Analysis of mixture.		Illuminating value per 5 cub. ft.	
Hydrogen.	Acetylene.	Argand.	No. 4 Bray.
92	8	Not measurable	
91	9	Not measurable	
88·5	14·5	4·1	1·7

so that luminosity would be destroyed in the argand by dilution with about 90 per cent. hydrogen, and in the No. 4 Bray with about 88 per cent.

The next point to be determined was whether the destruction of luminosity in the diluted acetylene flame was in reality due to dilution rendering it necessary to employ a higher temperature for the decomposition of the acetylene, or to other causes.

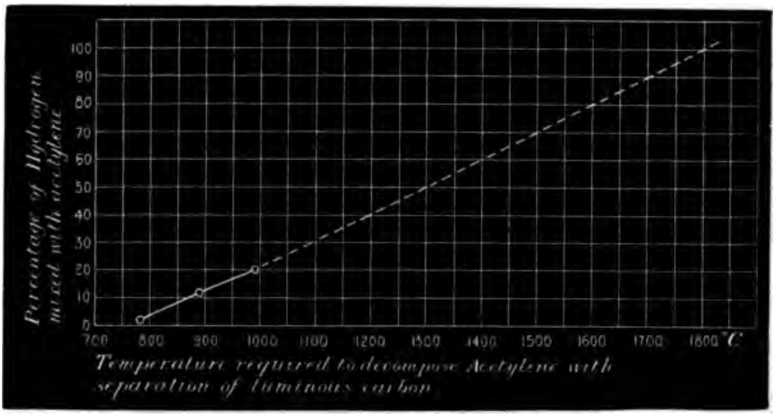
In order to do this, a tube made of specially infusible glass 4 mm. in diameter was taken, and the Le Chatelier thermo-couple was fitted into it in the same way as before, used with the platinum tube, and all air having been rinsed out by a current of the mixture to be experimented with, the gas was allowed to pass at a steady rate of flow through the tube, the point at which the thermo-couple was situated being steadily heated by the Fletcher blowpipe, whilst the temperature recorded on the scale was noted the moment that incandescent liberation of carbon commenced.

Percentage composition of gas.		Temperature necessary to cause deposition of carbon with luminosity.
Acetylene.	Hydrogen.	
100	0	780° C.
90	10	896
80	20	1000

It was found impossible to obtain a glass tube which would stand temperatures higher than this; but on plotting out the points so obtained, and which give a fairly straight line, it is seen that even if the increase in temperature only continues for increased dilution in the same ratio as shown in the experimental determinations, which is extremely unlikely, the reason of the destruction of luminosity in highly-diluted hydrocarbon gases is at once explained, as an increase of each 10 per cent. in the dilution would necessitate an increase of 100° C. in the temperature of the flame, and with 90 per cent. dilution a temperature of over 1700° C. would be required to bring about decomposition.

My reason for believing that it is highly improbable that when dilution is great it only requires the same increment in temperature to bring about decomposition as when the dilution is small, is that in all the work I have done on the effect of diluents upon luminosity, and also in Professor Percy Frankland's researches upon the same

FIG. 1.



subject, dilution with hydrogen and carbon monoxide acts regularly, and decreases the value of the illuminant in a direct ratio down to about 50 per cent., whilst when the degree of dilution exceeds 60 per cent. a rapid falling away in the luminosity takes place, a fact which I think points clearly to a regular *pro rata* rise of temperature being needed for increase in dilution up to between 50 and 60 per cent., whilst higher degrees of dilution need a far greater rise of temperature in order to bring about decomposition.

Moreover it would be manifestly incorrect to look upon the percentage of acetylene present in the gas issuing from the burner as being any guide to the degree of dilution existing at the point at which luminosity commences. As the two small streams of gas issuing from the holes in the union jet meet and splay themselves out into the flat flame, they draw in with them a considerable proportion of air, the quantity being governed by the pressure of the gas at the burner.

This can be clearly seen by the fact that a high value gas which burns from a union jet burner of a given size with a smoky flame, under a gas pressure of half an inch of water, will burn with a bright, smokeless, and rigid flame of greatly increased illuminating value when the pressure is raised to 2 in., whilst an ordinary coal gas of 16-candle value must be burnt from a flat flame burner at a pressure of about 0·75 in. if the best results are to be obtained, the increase in air drawn in, if the pressure rises to a much higher degree, diminishing the illuminating value.

Then, again, the area of non-luminous combustion in a mixture of gases like coal gas means that some at least of the hydrocarbons are consumed before the required temperature for their decomposition is reached, whilst the products of combustion formed in the lower part of the flame are mixed with the flame gases, partly by diffusion and partly by being drawn into it by the upward rush.

When a simple hydrocarbon like ethylene or acetylene is burnt alone, the whole of the heat required to bring about the decomposition has to be generated by the combustion, without decomposition, of a considerable proportion of the hydrocarbon, and this means considerable dilution at the spot where the luminosity commences, so that at the top of the non-luminous zone of an acetylene flame there is only some 14 or 15 per cent. of acetylene present, diluted with nitrogen, hydrogen, water vapour, and the oxides of carbon, whilst, with a mixture of 10 per cent. acetylene and 90 per cent. of hydrogen, in some cases little or no acetylene could be found at the top of the inner zone of the flame, it either having diffused with the hydrogen and been consumed, or polymerised to other compounds.

It is manifest that the luminosity of a flame will be governed, not by the percentage of acetylene in the gas, but at the point at which the temperature is sufficiently high to bring about decomposition.

If, instead of making a mixture of 90 per cent. hydrogen and 10 per cent. acetylene, the hydrogen is burnt at the end of an open platinum tube, which has a fine platinum tube passing up the centre to the top of the inner zone of the flame, and if the acetylene be passed into the flame at the rate of one volume for every ten of the hydrogen, not only do we obtain an intensely luminous, but a very smoky flame.

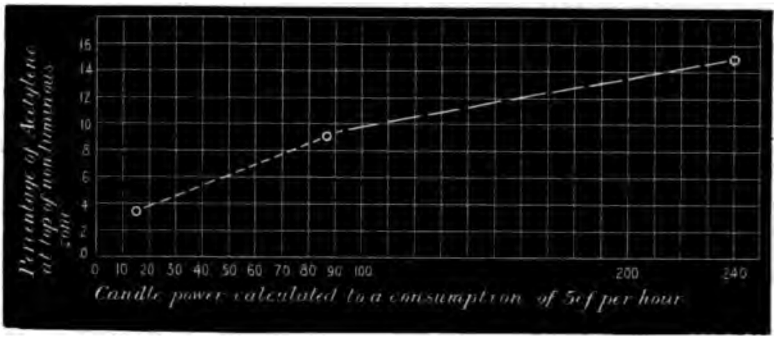
In this experiment the gases were issuing from their respective tubes at the same pressure, but the small tube soon choked from deposited carbon, and it was found that the same results could be equally well attained by drawing down the inner tube to the level of the hydrogen tube, and making the acetylene issue at a slightly higher rate of flow, which hurried it in a compact stream through the inner zone of the hydrogen flame.

In order to see if the percentage of acetylene present at the top of the non-luminous zone bore any ratio to the illuminating value of the mixture, experiments were made in which mixtures of hydrogen and acetylene were burnt at a small flat flame burner, and the percentage of acetylene was determined by gently aspirating out some of the flame gases from the top of the non-luminous zone.

Analysis of mixture used.		Acetylene at top of non-luminous zone.	Illuminating value of flame for 5 cub. ft.
Hydrogen.	Acetylene.		
65·5	34·5	3·72	14·0
43·5	56·5	8·42	87·0
0·0	100·0	14·95	240·0

On plotting out these results, they certainly seem to point to the fact that, with flames of the same size burning from the same burner, the light emitted by the flame is directly proportional to the percentage of acetylene present at the top of the non-luminous zone of the flame, provided always that the temperature is sufficiently high to complete its decomposition.

FIG. 2.



It is perfectly possible for the temperature of a flame to be so little above the point necessary to decompose the diluted acetylene that, whilst some decomposes and renders the flame faintly luminous, the larger portion burns without decomposition. A good example of this is to be found in the combustion of alcohol, the flame of which contains as much acetylene as is to be found in a good coal-gas flame, but which is practically almost non-luminous. If alcohol in a small dish be ignited, it burns with a faintly luminous flame, and if a bell-jar is placed over it, some of the products of combustion mingling with the flame still further cool it and render it non-luminous; but if now a stream of oxygen be introduced under the bell-jar the temperature of the flame is at once increased and becomes highly luminous, whilst a cold porcelain vessel held in the flame is coated with soot.

In all the experiments in which light was developed in heated tubes by the decomposition of acetylene, the glow of the carbon was red and lurid, the light emitted being of the same character and appearance as that developed by the combustion of potassium in carbon dioxide, and entirely lacking the pure white incandescence of the acetylene flame as burnt from a flat-flame burner.

This may be due to the fact that in the open flame the temperature of the carbon particles is presumably due to three sources of heat:—

A. Heat derived from the decomposition of the acetylene molecule.

B. Heat derived from the combustion of hydrogen, carbon monoxide, and some hydrocarbons in the flame.

C. Heat derived from the combustion of the carbon particles themselves,

whilst in the tube experiments the heat of the walls of the tube and the heat of decomposition alone are acting, and it is evident that the intensity of the heat finding its way through the walls of the tube will be very different to that exercised by the walls of burning gas which enclose the luminous portion of the flame, and there can be but little doubt that the temperature of the carbon particles will vary enormously with the rate at which the acetylene decomposes, as the more quickly the action takes place, the greater will be the localising action upon the heat evolved, and the higher the incandescence of the carbon particles.

That this is so seems certain from the whiteness of the flash of light emitted when the acetylene is detonated, and experiments were made in order, if possible, to gain an idea as to how much of the incandescence of the carbon particles was due to the endothermicity of the decomposing acetylene, and how much to the action of heat and combustion on the carbon particles after formation.

In order to do this, a non-luminous flat flame of large size was desired, and was obtained by using coal gas de-illuminated by slowly passing it through bromine, well washing with sodic hydrate solution and water, and then passing it through strong sulphuric acid, the gas so treated having an illuminating value of 1·2 candles for 5 cubic feet when burnt in the London argand at such a rate as to give a 3-in. flame, whilst in a fish-tail burner it gave a non-luminous flame. This gas gave on analysis the following percentage composition:—

Carbon dioxide	0·00
Unsaturated hydrocarbons	0·00
Carbon monoxide	5·50
Saturated hydrocarbons	33·28
Hydrogen	55·25
Nitrogen	5·49
Oxygen	0·48
	100·00

so that its combustion would give practically the same temperature and flame reactions as those in an ordinary gas flame.

A very fine platinum tube was now obtained, closed at one end, and with five minute holes bored in a line close to the sealed end, and this having been so arranged that the holes were buried in the flame just at the top of the inner zone, acetylene was then gently allowed to flow through them into the flame.

At the points where the acetylene issued into the flame, small areas of intense luminosity were produced, whilst the liberated carbon streaming up between the flame walls of the upper zone produced dull red bands of very low luminosity. It may be suggested that the carbon particles supplied in this way to the flame may have agglomerated and formed masses larger than those produced in the ordinary way, but I do not think this, as they were completely consumed, and no smoke escaped from the crown of the flame, whereas if a flat flame is interfered with in such a way as to cause the carbon particles to roll themselves together, smoking of the flame is produced.

I think the inference to be drawn from this experiment undoubtedly is that it is the heat of decomposition which gives the high incandescence and light emitting value to the carbon particles, and that the temperature of the combustion of the other flame gases and finally of the carbon itself plays but a secondary part.

In considering these results, it seems remarkable that if acetylene owes its power of rendering hydrocarbon flames luminous to its high endothermic properties, that cyanogen, which is still more endothermic, should burn under all conditions that have at present been tried with a non-luminous flame.

		Heat of formation.
Acetylene	C_2H_2	−47,770
Cyanogen	C_2N_2	−65,700

It is clear that if the rapidity of decomposition localises the heat evolved to the products of decomposition, and that this renders the liberated carbon particles incandescent, whilst the hydrogen plays at best a very subsidiary part, it ought not to matter whether it be hydrogen or nitrogen which is combined with the carbon.

Berthelot showed that cyanogen like acetylene could be detonated by a small charge of mercuric fulminate, but he notes that the test is not always successful, which points to the decomposition of this body requiring a greater expenditure of energy to break up the molecule than is the case with acetylene, and known facts would lead us to expect that this would be the case, as although exothermic compounds become less and less stable with rise of temperature, endothermic bodies on the other hand become more stable, and the endothermicity of cyanogen being greater than that of acetylene, would lead one to expect that temperatures which would decompose acetylene would have no effect on cyanogen, and that, as during the combustion of cyanogen, the liberation of nitrogen would probably have a diluting and cooling action, the cyanogen would burn directly without liberating any carbon which could emit light.

In order to see if the temperature of the cyanogen flame when burnt from an ordinary flat flame burner differed much from that of hydrocarbons when consumed in a flame of the same size and kind, the temperatures were experimentally determined by the same method employed, and in the same parts of the flame as had before been done with acetylene, ethylene, and coal gas.

Portion of the flame.	Temperature.
Centre of inner zone	1377° C.
Top of inner zone	2085
Near top of outer zone	1645

Showing that the cyanogen flame was actually hotter than the acetylene and ethylene flames, and about the same as the coal gas flame, but that the heat was differently distributed, the inner zone being far hotter than in the other gases, whilst the maximum temperature of the flame was at the apex of the inner zone, instead of being nearer the top of the flame.

An experiment was now made to ascertain if it were possible to decompose cyanogen with luminous deposition of carbon, by passing it through a hard glass tube heated by means of the blowpipe; but at the highest temperature attainable no trace of any deposition of carbon took place, showing how far more stable cyanogen is under the influence of high temperatures than acetylene.

The structure and characteristic appearance of the cyanogen flame have been explained by Smithells* and Dent, who conclude that the inner zone of peach blossom tint is caused by the combustion of the cyanogen to carbon monoxide and nitrogen, whilst the outer blue cone is formed by the oxidation of the monoxide to dioxide, the green fringe to the outer cone being attributed to the presence of small quantities of oxides of nitrogen; and if this explanation be accepted, it is clear that we could not obtain luminosity in the portion of the flame immediately above the inner zone, as all cynogen has been destroyed without decomposition before that point is reached. It is conceivable, however, that although no luminosity can be detected in a cyanogen flame, and although the temperature which can be obtained in a glass tube is insufficient to break up the compound with luminous separation of carbon, yet if cyanogen could be heated to a considerably higher temperature, it might be possible to decompose it in such a way as to develop luminosity.

In order to try this point, a hydrogen flame was burnt from the end of an open platinum tube 9 mm. in diameter, and a thin platinum tube 2·5 mm. in diameter was passed up through the broad tube to the apex of the inner zone, and a slow stream of cyanogen was admitted, with the result that the flame at once became luminous, and on surrounding the hydrogen flame with an atmosphere of oxygen to increase the temperature, the luminosity was considerably increased.

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* 'Chem. Soc. Jour.,' 1894, p. 603.

This experiment at once explains the cause of the non-luminosity of the cyanogen flame, and shows that it is purely a question of temperature, and the probabilities are that, burnt in a flame which gave sufficient heat to rapidly decompose it, nearly as high an illuminating value as that of acetylene would be obtained.

I think the explanation of the apparent anomaly of the cyanogen flame having a higher temperature than the acetylene and ethylene flames, is to be found in the fact that the molecules of cyanogen are consumed without previous decomposition, so that the heat absorbed during the formation of the cyanogen is added to the heat of combustion, and raises the average temperature of the flame, whereas with acetylene the instantaneous decomposition of the molecule before combustion confines the heat evolved to the liberated products, and the average temperature of the flame is but little more than the heat of combustion.

If the luminosity of a hydrocarbon flame is principally due to the localisation, during intensely rapid decomposition, of the heat of formation in the products, the illuminating values of such hydrocarbon gases as contain two atoms of carbon in the molecule should bear a simple ratio to their heat of formation. The gaseous hydrocarbons are—

Hydrocarbon.	Composition.	Heat formation at constant pressure.
Ethane	C_2H_6	+25670
Ethylene	C_2H_4	− 8000
Acetylene	C_2H_2	−47770

and although they may undergo many changes in the flame, they will all ultimately be reduced to carbon and hydrogen again before the full luminosity of the flame is developed.

When the acetylene into which these hydrocarbons is converted by heat is decomposed, the action takes place with such enormous rapidity that one would expect the heat evolved to simply divide itself amongst the liberated atoms, so that the question of specific heat at high temperatures may be omitted.

With exothermic compounds like ethane, considerable heat will have to be developed by its own combustion before it is converted into the acetylene, which, by its decomposition, endows the flame with luminosity, and if we take the ethane and call its light producing energy 1, we can then obtain a ratio of such energy for the other hydrocarbons available for distribution amongst the products of decomposition.

$$\text{Ethane} \ldots\ldots\ldots\ldots\ldots \quad \frac{25670}{25670} = 1$$

$$\text{Ethylene} \ldots\ldots\ldots\ldots\ldots \quad \frac{25670+8000}{25670} = 1{\cdot}31$$

$$\text{Acetylene} \ldots\ldots\ldots\ldots \quad \frac{25670+47770}{25670} = 2{\cdot}86$$

These ratios must now be divided amongst the atoms liberated at the moment of decomposition from the molecule, and we thus obtain the ratio :—

$$\begin{array}{ccccc} C_2H_6. & & C_2H_4. & & C_2H_2. \\ \frac{1}{8} & : & \frac{1{\cdot}31}{6} & : & \frac{2{\cdot}86}{4} \end{array}$$

or

$$1 : 1{\cdot}74 : 5{\cdot}72$$

The determination of the illuminating value of a gas becomes more and more difficult the higher its illuminating value, owing to the cooling effect of the small burners that must of necessity be used in order to ensure complete combustion. Dr. Percy Frankland[*] assigned the illuminating value of 35 candles to ethane as the mean of four tests, which varied considerably amongst themselves, and, adopting his figure, the calculated illuminating values for the ethane, ethylene, and acetylene would be :—

	Illuminating value.	
	Calculated.	Found.
Ethane	$1 \times 35 = 35$	35
Ethylene..........	$1{\cdot}79 \times 35 = 60{\cdot}9$	68·5
Acetylene	$5{\cdot}72 \times 35 = 200{\cdot}2$	240

figures which are far nearer the experimental ones than could have been expected, considering the crude character of the calculation and insufficient data, which leads to omitting altogether such important factors as the amount of gas consumed to bring about the requisite temperature of decomposition, the specific heat of the products, and the thermal value of the change from gaseous to solid carbon, and are of no value except as showing that a ratio does exist between heat of formation and illuminating value.

Methane is the only other gaseous hydrocarbon of which the heat of formation is known, it being $+21750$, and as the molecule contains only 1 atom of carbon, 2 mols. have to be taken, and on calculating the probable illuminating value by the same method as was applied to the other hydrocarbons, we should have—

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[*] 'Chem. Soc. Jour.,' vol. 47, p. 237.

$$\frac{\dfrac{25670+\{25670-(21750\times 2)\}}{25670}\times 35}{10\times\frac{1}{8}} = 8{\cdot}4,$$

and the illuminating value, as determined by Mr. Lewis T. Wright, is 5·2; but here, again, we know by experiment that methane requires a very high temperature to bring about its conversion into acetylene and decomposition into carbon and hydrogen, and that a large portion of the gas must be burnt without decomposition to do this.

The facts which I have sought to establish in this paper are:—

1. That the luminosity of hydrocarbon flames is principally due to the localisation of the heat of formation of acetylene in the carbon and hydrogen produced by its decomposition.
2. That such localisation is produced by the rapidity of its decomposition, which varies with the temperature of the flame and the degree of dilution of the acetylene.
3. That the average temperature of the flame due to combustion would not be sufficient to produce the incandescence of the carbon particles within the flame.

In my paper on the action of heat upon ethylene, brought before the Royal Society this spring, I showed that the decomposition of ethylene into acetylene and simpler hydrocarbons was mainly due to the action of radiant heat, and was but little retarded by dilution, whilst I have shown in this paper that the acetylene so produced requires a considerable increase in temperature to bring about its decomposition when diluted, and it is possible with these data to give a fairly complete description of the actions which endow hydrocarbon flames with the power of emitting light.

When the hydrocarbon gas leaves the jet at which it is being burnt, those portions which come in contact with the air are consumed and form a wall of flame which surrounds the issuing gas. The unburnt gas in its passage through the lower heated area of the flame undergoes a number of chemical changes, brought about by the action of radiant heat emitted by the flame walls, the principal of which is the conversion of the hydrocarbons into acetylene, methane, and hydrogen. The temperature of the flame quickly rises as the distance from the jet increases, and a portion of the flame is soon reached at which the heat is sufficiently intense to decompose the acetylene with a rapidity almost akin to detonation, and the heat of its formation, localised by the rapidity of its decomposition, raises the liberated carbon particles to incandescence, this giving the principal part of the luminosity to the flame; whilst these particles, heated by the combustion of the flame gases, still continue to glow, until finally themselves consumed, this external heating and final combustion adding slightly to the light emitted.

Any unsaturated hydrocarbons which have escaped conversion into acetylene before luminosity commences, and also any methane which may be present on passing into the higher temperatures of the luminous zone, become converted there into acetylene, and at once being decomposed to carbon and hydrogen, increase the area of the light-giving portion of the flame.

My thanks are due to Mr. F. B. Grundy for the help he has given me in the work entailed by this paper.

IV. "A possible Explanation of the two-fold Spectra of Oxygen and Nitrogen."

By E. C. C. BALY, A.I.C., 1851 Exhibition Scholar in University College, London. Communicated by Professor RAMSAY, F.R.S. Received February 27, 1895.

(Abstract.)

The two spectra of oxygen are shown to be of a different nature. They behave differently, and reasons are given for their being in all probability the spectra of different gases. They may either be two spectra produced by different vibrations of the oxygen molecule, or they may be the spectra of two different modifications of oxygen, or the spectra of two distinct gases resulting from a dissociation of oxygen, a combination of which is called oxygen.

It appeared worth while to undertake experiments with a view of testing the last of these. Oxygen was sparked in an apparatus similar to that used by Professor J. J. Thomson in his experiments on the electrolysis of steam. Hollow platinum electrodes were used, each one of which was connected with a Sprengel mercury pump. In the first experiments, the distance between the electrodes was 35 mm., and the highest pressure compatible with the appearance of the two spectra was made the starting point of the experiments. In these first experiments it was 380 mm. The density of the oxygen before sparking was determined, and taken as a test of its purity. The fractions obtained from the anode and cathode were weighed, and the results are given. They follow the lines of J. J. Thomson's results, inasmuch as with long sparks a lighter fraction was obtained at the cathode, and with short sparks a heavier fraction. The fractions from the anode were not so definite as from the cathode, though the difference was in the right direction. The probable maximum error of weighing was 0·0001 gram. This meant exactly one in the second decimal place of the density obtained. The general accuracy of the results may be gauged from the densities of unsparked oxygen obtained.

Density of cathode fraction with long sparks.	Density of oxygen unsparked.	Density of cathode fraction with short sparks.
15·78	15·88	16·00
15·79	15·87	16·01
15·80	15·89	16·02
15·79	15·88	16·04
	15·88	16·06
		16·05

Mean of results of other observers = 15·887.

Density of cathode fraction from oxygen, previously for three days fractionated with short sparks, 15·75.

The experiments are still in progress.

V. "On the Question of Dielectric Hysteresis." By Alfred W. Porter, B.Sc., Demonstrator of Physics, University College, London, and David K. Morris, 1851 Exhibition Scholar, University College, London. Communicated by Professor G. Carey Foster, F.R.S. Received March 2, 1895.

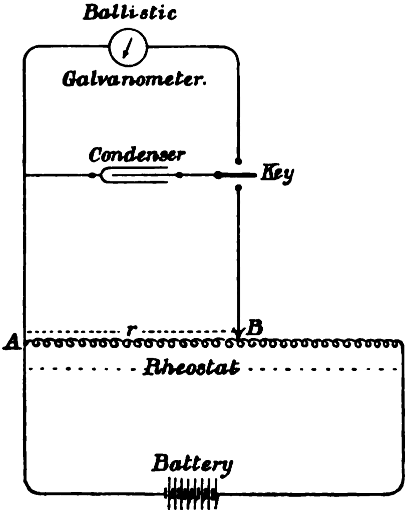
The condenser on which the following experiments were made is the one referred to in a paper by one of us read before the Royal Society on June 1st, 1893 ('Roy. Soc. Proc.,' vol. 54, p. 7). It is a 5-microfarad condenser of tinfoil and paraffined paper, made by Messrs. Muirhead. In the paper referred to it was shown that when it is allowed to discharge itself through a coil containing induction the rate of dissipation of energy (calculated from the damping of the oscillations that occur) is greater than that due to the resistance of the outside circuit: the additional dissipation being equal to what would have taken place if about 59 ohms had been added to the circuit resistance.

The following experiments were made with the idea of ascertaining whether this additional dissipation is the result simply of viscosity in the dielectric of the condenser or to true hysteresis of the charge with respect to the potential difference between the condenser plates. A sharp distinction is not always made between the two phenomena; it cannot be too clearly borne in mind that, on the one hand, viscosity is a "time" effect—*i.e.*, it depends on the rate of change of the variables; while, on the other hand, the phenomenon of hysteresis does not in any way involve the rate at which the changes in the quantities are made.

Rapidly performed series of cycles, such as occur during an oscillatory discharge, are hence unsuited for discriminating between the two causes: to test whether true hysteresis exists it is essential to so arrange the experiment that all viscous effects shall have had time to subside before any measurement of charge is made. Hence the interesting experiments made during the last three years by Riccardo Arnò,[*] in which a dielectic cylinder begins to rotate when placed in a rotating electrostatic field, as well as the experiments made by P. Janet,[†] and by one of us using oscillatory discharges, do not serve as test experiments on this question.

The arrangement which we adopted to test for hysteresis is as follows:—

FIG. 1.



A battery (E.M.F. = 11 volts) is connected permanently in simple circuit with the ends of a rheostat of 850 ohms resistance. The condenser terminals can be put in contact by means of a two-way switch, with one end A, and an intermediate point B of the rheostat.

The position of the point B is capable of continuous adjustment, and hence also is the difference of potential between the condenser terminals. The charge that the condenser possesses at any time can be ascertained by discharging it (by means of the switch) through a ballistic galvanometer.

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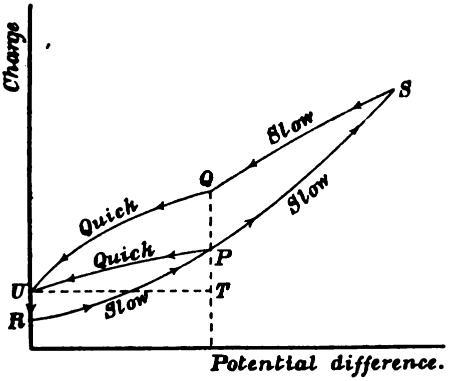
[*] Published in the 'Rendiconti della R. Accademia dei Lincei,' October 16, 1892, p. 284, April 30, 1893, p. 341, November 12, 1893, p. 260, March 18, 1894, p. 272, June 17, 1894, p. 585, November 18, 1894, p. 294. For translations *vide* 'The Electrician,' March 3, June 23, December 29, 1893, and January 18, 1895.

[†] "Sur les Oscillations Électriques de Période Moyenne," 'Journ. de Phys.,' August, 1893. For other recent literature on the subject *vide* A. Hess, "Sur les Diélectriques Hétérogènes," 'Journ. de Phys.,' April, 1893; Steinmetz, 'The Electrician,' April 8, 1892 (from the 'Electrical Engineer' of New York).

Since the methods for testing for hysteresis consist in putting the thing tested through cyclic series of states, it is essential that any change of state involved in the measuring operations should itself form part of a cycle. This was accomplished by making the cycles in the manner indicated in Fig. 2, which has been drawn as it might appear if considerable hysteresis were present.

FIG. 2.



Commencing at the point R in the cycle (*i.e.* with no difference of potential between the terminals), the potential difference is gradually increased until the point P on the curve is reached; the condenser is then discharged; the portion of the curve PU is traced during the "instantaneous" discharge, and the galvanometer indicates the loss of charge PT, which includes whatever viscous flow takes place during the time of throw of needle (2½ seconds). While the galvanometer is still in connection with the condenser a further viscous flow takes place, until, if the cyclic state has been set up, the starting point R is arrived at. Before allowing the switch to again connect the condenser to B (Fig. 1) the rheostat is unwound so as to bring B back to A; there is then no potential difference between B and A, and the condenser receives no charge when the switch completes connection. After this connection has been made the potential difference is again gradually increased, but to a higher value than before—to S (say); it is gradually diminished till it has the same value as at P; we thus arrive at a point Q; the condenser is then discharged through the galvanometer; the throw of the needle measures the loss of charge QT, which includes the same viscous effect as before; the point U is reached during the throw of needle: after which, a further viscous flow takes place, until (if the cyclic state be set up) the point R is returned to once more.

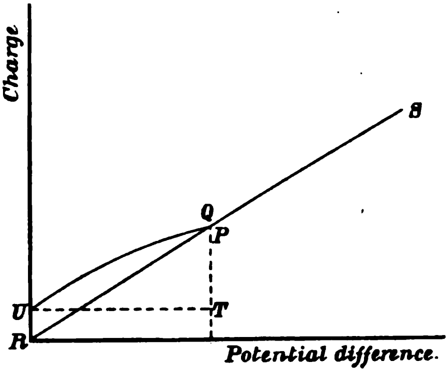
The contact B is moved back to A while the condenser is still detached from it; and this series of operations is then many times repeated.

If the dissipation of energy in the condenser is due to hysteresis, then, as in the magnetic analogue, we know that the cyclic curve will embrace an area, and the point Q will not coincide with the point P; and the converse of this is also true if time has been given for viscous effects to subside.

If, on the other hand, experiment shows no difference in the charge when at Q from what it is at P, there can be no hysteresis, unless of an amount so small as to elude this method of detecting it.

The following figure (Fig. 3) shows the kind of curve that will be traced if hysteresis be absent.

FIG. 3.



The experimental results are given in Table I.

The point P was so chosen that the charge was then almost exactly half of the maximum charge. The duration of a cycle of operations was never less than five minutes. The galvanometer throws were read by a telescope and scale, and are expressed in centimetres. As both galvanometer mirror and telescope are of fine quality there is no difficulty in detecting a tenth of a millimetre on the scale.

It will be observed that the throws fall off gradually throughout the experiment in consequence of gradual failure of the battery power; but as this affects both series to the same extent it has no influence on the ratios. Individual values of the ratio Throw at Q/Throw at P differ amongst themselves by small amounts, being below unity in 11 cases, above in 12, and equal to unity in one case.

Table I.

Discharge at P.		Value of $q-p$.	Discharge at Q.	
Time of discharge.	Galvanometer throw (p).		Time of discharge.	Galvanometer throw (q).
hrs. m.			hrs. m.	
11 27	52·61	−0·11	11 33	52·50
11 37	52·72	+0·06	11 44	52·78
11 48	52·77	−0·01	11 52	52·76
11 56	52·71	−0·01	12 4	52·70
12 6	52·63	+0·02	12 10	52·65
12 12	52·61	+0·03	12 17	52·64
12 21	52·53	+0·06	12 25	52·59
12 28	52·45	+0·06	12 32	52·51
12 36	52·49	+0·05	12 44	52·54
12 46	52·53	−0·03	12 50	52·50
12 53	52·48	−0·03	12 58	52·45
12 60	52·43	−0·02	1 4	52·41
2 39	52·04	+0·02	2 45	52·06
2 54	52·00	−0·02	2 58	51·98
3 4	51·98	−0·03	3 11	51·95
3 14	51·95	−0·06	3 18	51·89
3 21	51·92	+0·04	3 25	51·96
3 27	51·83	−0·05	3 31	51·78
4 32	51·70	−0·07	4 36	51·63
4 39	51·55	+0·04	4 42	51·59
4 44	51·56	+0·02	4 48	51·58
4 55	51·55	+0·02	5 00	51·57
5 4	51·53	+0·03	5 8	51·56
5 11	51·53	+0·00	5 16	51·53

	At P.	At Q.
Mean of first 12 throws……	52·580	52·586
Mean of second 12 throws….	51·762	51·757
Mean of all 24 throws……..	52·1710	52·1715

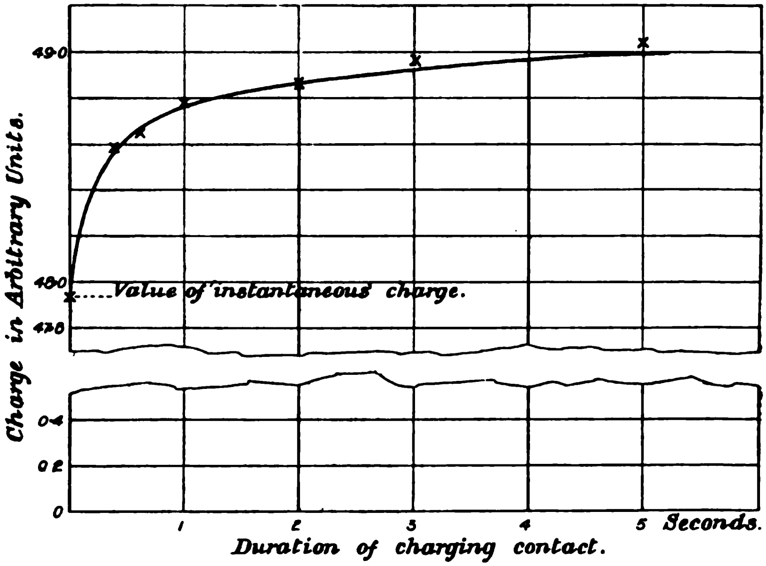
The ratio derived from the first twelve values is $1 + \frac{1}{8760}$, that derived from the second twelve values is $1 - \frac{1}{10350}$. The best values of the ratio derived from all 24 values approximates still more closely to unity.

Hence the position of the point Q in the diagram is indistinguishable from the position of the point P; and we may conclude that when time has been given for viscous effects to subside the charge of the condenser is the same for a given value of the potential difference, whether that value has been arrived at from higher or from lower values than itself.

Since, until further experiments be made, this conclusion can only be taken as applying to the condenser when under the conditions of this experiment, it is necessary to form an idea as to the highest value of the electromotive intensity which was produced in the dielectric. Only a very rough estimate of its value can be made without dismounting the condenser. If we assume that the thickness of the paraffined paper is about ·01 cm., then with 11 volts potential difference between the plates, the electromotive intensity equals 1,100 volts per cm., or about 3·7 electrostatic units. If we take the dielectric constant of paraffined paper as 2, the corresponding electrostatic induction is 7·4 electrostatic units. It may be mentioned that in the experiments of Signor Arnò the induction ranged from ·03 to 14·58 electrostatic units.

In order to give an idea of the viscous effects which come into play in the same condenser, we have ascertained the variation of the charge which it takes up on the application of an unvaried E.M.F. to its terminals for different intervals of time. The results are plotted in Fig. 4. The time constant of the rise of charge, calculated from the capacity of the condenser and the resistance of the circuit through which it was charged, was less than $\frac{1}{20000}$ of a second: hence in 7 times $\frac{1}{20000}$ of a second the charge would have risen to within $\frac{1}{10}$ per

FIG. 4.—Relation between Charge of Condenser and the Duration of Application of Charging Electromotive Force.



cent. of its final value if dissipation of energy had taken place only in the outside circuit.

Thus, while the condenser here experimented upon exhibits marked viscous effects, yet we can detect no hysteresis.

The foregoing experiments were conducted in the Physical Laboratory of University College, London, and our thanks are due in many respects to Professor G. Carey Foster in connection with them.

VI. "On the Changes in Movement and Sensation produced by Hemisection of the Spinal Cord in the Cat." By CHARLES DEVEREUX MARSHALL, F.R.C.S. Communicated by Professor V. HORSLEY, F.R.S. Received January 31, 1895.

(Abstract).

The present research was undertaken in order to determine more exactly the nature and origin of epileptiform convulsions and the paths in the spinal cord by which both afferent and efferent impulses are transmitted.

The method employed was as follows:—

Hemisection of the spinal cord was performed in the lower dorsa region on the right side, the animals being anæsthetized with ether and strict antiseptic precautions being observed.

The animals were kept alive for different periods of time after the operation, and the effects produced on the voluntary movements, sensibility, and the reflexes were carefully noted.

After death the spinal cords were at once removed, and after hardening and staining, by Marchi's method, both the lesion and the tracts of degeneration were investigated by microscopic examination. There were 16 experiments performed.

The following results are briefly what were obtained.

(*a.*) *Motion.*—After hemisection of the cord there is immediate paralysis of the limb below, and on the same side as the section, this remains for a time, and then gradual recovery takes place to a greater or less extent; sometimes the recovery is so complete that it is with difficulty that one can determine which was the paralyzed limb. At other times more or less permanent weakness is left so as to cause a limp when walking, foot-drop is not infrequently seen and the animal does not appear to appreciate correctly the position that the leg occupies.

(*b.*) *The Reflexes* are, as a rule, greatly exaggerated below and on the same side as the lesion, and sometimes continue so for a long period; in many cases they get less as time goes on; occasionally they are not so well marked as on the opposite or uninjured side.

(*c.*) *Sensation.*—This is always interfered with on the side of the lesion. Painful sensations, such as those produced by the prick of a pin, or by touching the foot with a piece of wire which is slightly heated, appear to be felt on both sides, and this was well seen in the monkeys which were used as control experiments; but the evidence seems to show that these painful sensations are more quickly felt on the non-paralyzed than on the paralyzed side, and also that the animal does not localize the painful spot on the injured side with anything like the precision that it does on the unaffected side.

Tactile sensations, such as those produced by the presence of a small clip applied to the skin, are only felt on the non-paralyzed side; the animals were frequently seen to remove the clip from this side, but unless their tails touched it when placed on the paralysed limb, or their attention was directed to its presence in some such way, no notice whatever was taken of it.

The same result was seen with the cold-water test. When water was brought in contact with the feet, the unaffected limb was at once withdrawn and the water licked off it, while on the opposite side the foot was allowed to stand in a pool of water without any notice being taken of it.

Microscopic Examination of the Spinal Cords.

The portions of the cords containing the lesion were carefully examined for the purpose of ascertaining their exact extent, and a full description is given in the paper.

The following is a short description of the degenerations found in the cord.

(1.) *Descending Degenerations.*—These degenerations were confined almost entirely to the side that was cut, and they occupied the direct and crossed pyramidal tracts; a few scattered degenerated fibres were seen in the antero-lateral tracts of both sides; this was especially marked in the cases where a small part of the opposite half of the cord had become accidentally injured.

(2.) *Ascending Degenerations.*—These degenerations were not entirely confined to the side of the lesion.

The tracts that contained most of the degenerated fibres were the column of Goll, the direct cerebellar tract and the antero-lateral tract. Some degenerated fibres were usually found in the opposite column of Goll, and also in the antero-lateral tract of the uninjured side.

VII. "On the Analysis of Voluntary Muscular Movements by certain new Instruments." By WILLIAM R. JACK, M.D., B.Sc. Communicated by Professor J. G. MCKENDRICK, F.R.S. Received January 25, 1895.

(From the Physiological Laboratory of the University of Glasgow.)

(Abstract.)

The object of this investigation was to determine the greatest speed of which the voluntary muscular movements were capable, and how far the speed was influenced by age and education. It was decided to limit the research to a study of the movements of the fingers, as being the easiest parts to examine of the muscular system. The writer has to acknowledge his indebtedness to Professor McKendrick for much valuable assistance and advice.

The first instrument used was one devised by Professor McKendrick, and figured at p. 78 of his 'Life in Motion' (first edition, 1892). It consisted essentially in a tuning fork, carrying a smoked microscopic slide, and set in motion by a bichromate cell. With all its loads, the fork made 117 double vibrations per second, as determined by the chronograph. Lines drawn upon the attached slide perpendicular to the plane of vibration were, therefore, thrown into waves, each of which represented the space passed over in 1"/117. The different movements investigated in this way were the following:—(1.) The greatest velocity attainable by the single contraction of one finger, to which a finely-pointed needle was affixed. (2.) The greatest velocity attainable by the single contraction of the fingers in combined action. For this purpose the needle was fixed in a penholder, and held like a pen, care being taken to see that there was no movement of the wrist, but only of the fingers. (3.) The greatest velocity obtainable in ordinary writing. In this way, therefore, the velocities of a series of movements increasing in complexity could be compared. The velocity of curves and of curvilinear figures was also compared with that of straight lines.

Twenty-three normal and two pathological cases were examined. The normal cases were divided into three classes: those of special manual education (musicians), those of average manual education (the ordinary educated classes), and those of inferior manual education (working men), in whom the hands were accustomed only to coarse movements, and the fingers had no special training. Of the first class there were five examples; of the second and of the third, nine. The ages varied from 18 to 62.

While the tracings were being made the hand rested on a little table at the height of the recording slide. It was found that the velocity of the more complicated movements could be increased to some extent by practice. Only a few preliminary tracings, to accustom the hand to the instrument, were therefore allowed to each subject, in order that all might be as much as possible on the same footing. Full details of the velocities attained in each case, and figures of the apparatus used, are given in the original essay. In this abstract only a statement of the results attained will be made.

It is not contended that the figures there given represent the absolute velocities of the movements made, for the retarding influence of friction on the slide has to be allowed for, and the increase in the space travelled over through the extension of the needle beyond the end of the fingers. But, as all the subjects were under the same conditions, a comparative estimate of the velocities may be arrived at.

Beginning with the influence of training upon the velocity of the movements, the pen-movement was first studied, and the rate at which a distance of 3 cm. in the middle of the slide was traversed was taken as the standard of comparison. Unfortunately no tracings of this movement were taken from musicians, but, on comparing the average of the tracings derived from those of average and those of inferior manual education, the following conclusions were arrived at.

(1.) That in those of inferior manual education (whose two hands were both untrained) the velocity is equal in both hands (3·4″/117 for the right and 3·5″/117 for the left hand).

(2.) That in those of average manual education the velocity is greater in the right hand, which has been trained (as in writing) than in the left, which has not (2·9″/117 for the right and 3·6″/117 for the left hand), an exception being found in the case of the author, both of whose hands had been pretty equally trained, in which case both had the same velocity (3″/117).

(3.) That the velocity is greater in the right hand of those of average education than in the right hand of those of inferior education.

(4.) That the velocity in the left hand (which has been little trained) of those of average education is practically identical with that in the left hand of those of inferior education.

The velocity of contraction of a single finger was next examined with the following results:—

(1.) That the velocity is equal, or nearly so (2″/117) in the first and second fingers, and is greater than that in the third and fourth, which have also a nearly equal velocity. This may be due to the special arrangement of the tendon of the extensor communis digitorum for the third finger, and for the fourth to the awkward position in which it was placed.

(2.) The velocity of each finger is practically identical for the two hands.

(3.) That the velocity of the finger-movements is not appreciably affected by manual training. The most rapid single contraction of the fingers of a trained pianist is very slightly, or not all, faster than that of a working man.

(4.) The velocity of movements of flexion is on the average slightly greater than that of movements of extension, although in two of the eight cases examined the velocities were identical.

(5.) The velocity of the finger-movements, as a whole, is greater than that of the pen-movements.

In examining the more complicated movements of writing it was found:—

(1.) That the average velocity is practically the same in musicians and in those of average education, their training in this regard being nearly equal.

(2.) That the velocity in the untrained working-classes is much less than in the two former.

(3.) That the velocity in all classes is much less than in the pen-movements.

(4.) That the curved parts of letters and figures are more slowly formed than the rectilinear parts, and that the velocity of a curve varies, roughly speaking, with the radius of curvature.

The difference, then, between education and the want of it is greatest in writing, less in pen-movements, and scarcely noticeable in the simple finger-movement. And the average velocity for all classes is least in writing, much greater in pen-movements, and greatest in finger-movements. The velocity therefore diminishes, and the difference between the various classes increases as the movements become more complex. The more nearly they approach to a simple muscular contraction, the less is the difference noticeable; though it would, doubtless, be found that the musician is able to repeat a series of simple muscular contractions much more rapidly than one with untrained fingers.

The influence of age upon the velocity of the movements was next investigated, and it was found, with regard to writing:—

(1.) That the velocity of the movements of writing becomes slower with advancing age.

(2.) That it is greatest between the ages of 20 and 29, and decreases with every decade thereafter.

(3.) That this decrease is greater in the uneducated than the educated.

With regard to pen-movements it was found:—

(1.) That the decrease in velocity is less marked than in the case of writing.

(2.) That the velocity is greatest between the ages of 20 and 29.

(3.) That the difference in the rate of decrease between the educated and uneducated classes is not so marked as in the case of writing.

With regard to finger-movements it was found that they retained nearly the same velocity for all classes between the ages of 20 and 50; and that in the one case of a man over that age (a labourer, æt. 62) there was a decided decrease.

In the two pathological cases investigated, one of lateral sclerosis in a man of 41, and one of tremor of the hands, following upon syphilis, in a man of 50, similar results were obtained. For the complex movements of writing were most seriously retarded, the pen-movements less, and the finger-movements least of all.

It appears, then, that as a movement increases in complexity, and involves in its performance the associated action of a greater number of muscles, its velocity diminishes, and the influence of education becomes more distinctly manifest. And as complex movements require a longer education for their rapid performance, so they appear to become sooner defective than the simpler movements. For it is in writing that the retarding effect of age is most apparent, while it is least so in the finger-movement.

In the second part of the investigation it was desired to obtain tracings from a larger series of contractions than could be registered upon a microscopic slide. For this purpose new instruments were required, and owing to the long delay in making these, but few experiments have been recorded. The instrument finally adopted consisted in a long steel bar, held firmly in an iron clip, and carrying in a clamp attached to one end a smoked glass plate 6 inches square. It was set in motion by an electro-magnet through which passed the current from a storage battery, and, as determined by the chronograph, it made fifty-four double vibrations per second. At Professor McKendrick's suggestion it was determined rather to investigate with this instrument the phenomena of fatigue, a purpose for which he thought it very suitable. With this object, Mosso's ergograph was adapted to the instrument, the recording part of which could be pulled on rails slowly away from under the registering lever, which worked up and down with the movement of the weighted finger. Thus a series of contractions and relaxations, divided by the oscillations of the bar into fifty-fourths of a second, was registered on each plate. The plate could be taken out, and a new one substituted, beneath the lever, without stopping the movement of the finger. A key was interposed in the circuit to shut off the current while the plates were being changed.

Four normal and two pathological cases were investigated, series of tracings being taken with a ½ kilo., a 1 kilo., and a 2 kilo. weight. They show in a very striking manner the diminution in height of the contraction, and the coincident diminution in its velocity, due to fatigue. The measurements which were taken of the rates of velocity at different parts of the tracings afford somewhat varying results, and the number of cases is too few for any definite conclusions to be drawn from them. But it would appear that while the diminution is gradual and uniform in the case of small weights, in that of larger weights it occurs, as a rule, more rapidly, and that the rate of diminution does not remain the same throughout the tracing.

VIII. "Experiments upon the Influence of Sensory Nerves upon Movement and Nutrition of the Limbs. Preliminary Communication." By F. W. MOTT, M.D., F.R.C.P., and C. S. SHERRINGTON, M.D., F.R.S. Received March 7, 1895.

In the 14th of the 'Leçons sur la Physiologie et la Pathologie du Système Nerveux,' Claude Bernard draws attention by experiments on the frog and on puppies to the degree of impairment in movement undergone by a limb that has been rendered insensitive by section of the sensory roots of its spinal nerves.

In a series of experiments carried out during the last eighteen months, we have examined the same thing in the monkey, using chiefly *Macacus rhesus*, and observing the animals for periods up to four months from the time of operation.* We propose to give here a brief account of the results obtained.

Our experiments deal separately with the lower limb and with the upper limb. The phenomena observed in the two limbs do not essentially differ, but are rather more marked and much more accessible to examination in the case of the upper limb.

I. *On Movement.*

(1.) *Effect of Section of the whole Series of Sensory Roots belonging to the Limb.*—By the "whole series" is meant in the brachial region from the 4th cervical to the 4th thoracic inclusive; in the lumbar from the 2nd to the 10th post-thoracic inclusive.

From the time of performance of the section onwards, as long as the animal may be kept, the movements of the hand and foot are practically abolished; the movement of grasping, which is so frequent and useful to the monkey, both with the hand and foot, never occurs at all in our experience. On the other hand, the movements at the elbow and knee, and especially the movements at the shoulder and hip, are much less impaired. The fore limb hangs from the shoulder partially flexed at the elbow; the hind limb is flexed at hip and knee. As the animal runs about it does not attempt to use the leg; the fore limb swings helplessly, with flexion at elbow and wrist and adduction at shoulder, in much the same position as if carried in a sling. The hind limb looks as if it were being held up so as to be kept off the ground while the animal runs on three legs; we are inclined to think that this appearance is deceptive, and that the position results from an equilibrium of the action of the muscles, in which purposive action on the part of the animal does not play a *rôle*. When the animal is allowed to climb a rope or the side of the cage, the fore limb swings more or less helplessly, and is not used for the climbing; similarly, the hind limb is kept more or less flexed at hip or knee, and is not used for the climbing. If the feeding-time be deferred, and an animal, in which the apæsthete* limb is an arm, be tested by offering it fruit after the sound arm has been secured behind the back, there is no attempt to use the apæsthete limb for reaching the food, but the neck is thrust forward in order for the mouth to seize it. If the fruit be placed in the hand of the apæsthete arm, the animal does not lift the hand, and appears quite unable to do so, even though encouraged. If, however, the hands of a tame normal monkey be secured behind its back, and, as it lies on the floor, fruit be placed near it, the fruit is usually taken at once with the foot; but if the leg is apæsthete the fruit cannot be taken, although in one monkey the attempt used to be made. The foot was rapidly thrust toward the fruit by extension of hip and knee, but the foot missed its object widely, *i.e.*, by several inches, and the digits were not moved, though the ankle appeared to be slightly plantar-flexed. The impairment of motility in the limb ensues immediately upon completion of the section; that is to say, directly the effects of the anæsthetic have passed off sufficiently to allow requisite examination of the animal's ability to move its limbs, the above-described inability is discoverable as fully developed as at any subsequent period. We have kept the animals alive for various periods up to and over three months, and there has been no obvious change in the condition, either in the direction of improvement or the reverse. In the case of the lower limb, after two or three months, the constant position of flexion of hip and knee, on two occasions, gradually induced a change in the muscles of the thigh, which prevented hip and knee being properly extended, even by passive stretching.

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* In all our operations the animals have been deeply anæsthetised with chloroform and ether.

As to the nature of the disturbance of motility in the limbs, one feature, namely, its peculiar topographical distribution, is salient and constant. The defect in motility increases from the attached base to the free apex of the limb; so that, for instance, while comparatively slight at the hip, it is successively greater at knee and ankle, and greatest (amounting as regards volition to absolute loss) in the digits.

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* Απὸ, *αἰσθάνομαι*, "deprived of sensation," in distinction from anæsthete, "devoid of sensation." We are indebted to Dr. Verrall, of Trinity College, Cambridge, for the suggestion of this term.

In this respect it curiously closely simulates the impairment of motility ensuing upon ablation of the limb region of the cortex cerebri; but it is, in the monkey, somewhat *more severe* than the impairment following cortical ablation.

We find, however, that forcible and rapid movements, even of the fine joints at the end of the limb, can be induced in the animals by causing them to "struggle"; for instance, while recovering from ether inhalation, or while trying to free themselves on being held awkwardly, the whole limb at all its joints may exhibit movements; but even under these circumstances it is only once or twice that we have seen "grasping" movements of the digits, although sharp extension of the digits is not nearly so infrequent.

We are led from these and other considerations, which will be detailed in a fuller paper, to conclude that *associated movements in the limb* ("Mitbewegungen") *are comparatively little impaired* by loss of the sensation from the limb in which they occur; but that the independent and more delicately-adjusted movements which employ preponderantly the smaller and more individualised muscular masses of the hand and foot, and serve to move the digits, especially the hallux and the thumb—in fact, just *those movements which are represented most liberally in the limb area of the cortex, are extremely severely impaired, and, in some instances, are abolished.* We say 'abolished' advisedly, because we are persuaded from our observations that, in the case of certain movements, *e.g.*, grasping movements of the hand and foot, opposition of pollex and hallux, the animal is rendered absolutely powerless to perform them, even under the strongest possible inducements. This conclusion has been gradually forced upon us. Although we are aware of the danger of introducing terms relating to consciousness into descriptions based almost solely on motor reactions, we believe that we cannot more lucidly state the condition of the animals than by saying that the volitional power for grasping with the hand, &c., had been absolutely abolished by the local loss of all forms of sensibility experimentally produced. Further, that this volitional power was lost immediately from the time of operation, and that there was not the slightest evidence of any recovery of it during the longest periods to which our observations extended (about four months).

This being so, it is natural to inquire what influence, if any, is exerted by the section of the posterior spinal nerve-roots of the limbs upon the reactions obtainable from the limb area of the cortex? That no diminution, but rather a slight increase of the excitability of the cortex, is the immediate result has been shown by one of us previously.* But the question remained, what will be the result when, for many weeks, the severance of the roots has led (as above shown) to disappearance from the limb of those very movements which the cortex, when experimentally excited, is especially able to produce? We have answered this, both by electrically exciting the cortex and by giving absinthe intravenously to produce epilepsy (Magnan). On exciting the cortex cerebri of the hemispheres in the appropriate regions for eliciting movements of the thumb, hallux, or digits, the responsive movements have been as easily elicited from the apæsthete limb as from the normal limb, and it has several times seemed to us rather more easily, that is to say, with a slightly less intensity of faradic current (the rate of interruption always remain-the same, $\frac{1}{50}''$).

As to the absinthe epilepsy, it always affected the apæsthete limb in a manner not distinguishably different from the normal limb. Convulsions sometimes started in the normal and desensitized limb simultaneously, sometimes a little earlier in one or the other; but no indubitable predominance or preference was shown by either limb. In a very few of our experiments (three) no movement was obtained in the apæsthete limb on excitation of the cortex; this was found to be explained subsequently by naked-eye degeneration of the pyramidal tract as revealed after hardening in Müller's fluid. This degeneration was due to injury accidentally inflicted upon the lateral column of the cord by the operation. The spinal tonus in the muscles of the apæsthete limb is undoubtedly much diminished.

These observations seem to us to point to the profound difference existing between the production of the finer movements of the limb in volition on the one hand, and by experimental stimulation of the cortex on the other. The fundamental importance of sensation for those finer movements of the limb, which are so especially well represented in the cortex of the ape, has by no authority been more forcibly emphasized than by Dr. Bastian. We think these experiments go even further than his arguments in pointing to the influence of sensation upon voluntary movement, inasmuch as they indicate that not only the cortex, but the whole sensory path from periphery to *cortex cerebri*, is in action during voluntary movement.

(2.) *Effect of Section of a single Sensory Root.*—In striking contradiction to the above-stated impairment of movement in the limb ensuing upon section of the whole series of its sensory nerve roots stands the effect of section of any one of the sensory nerve roots of the series singly and alone. In the latter case no impairment of movement at all results, or, at least, can with certainty be detected.

This is the case even when the largest and most important sensory root of the series is chosen for section; namely, in the upper limb the 8th cervical, and in the lower limb the 6th post-thoracic. (These are the nerves that supply the skin over the whole of the hand and foot respectively. It is to be remembered, however, that hand and foot respectively are each of them supplied with sensation by at least three sensory roots, the middle root covering the whole surface in each case.)

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* C. S. Sherrington, 'Phil. Trans.,' vol. 184, B, pp. 690, 691.

We attribute the fact that section of these large roots with their wide distribution over hand and foot produces so little appreciable effect, to the fact that the distribution of all the spinal nerves in the skin is an overlapping one. The extent of overlapping is great enough to prevent the section of any one nerve, even of the largest, producing actual anæsthesia of the skin in any part.

We further find that even if a field of absolute anæsthesia be actually produced by section, for instance, of the 7th, 8th, and 9th post-thoracic roots, or, in some cases, by section of the 7th and 8th cervical and 1st and 2nd thoracic roots, the impairment of movement resulting in the limb is comparatively slight. This is the more remarkable when the region deprived of sensibility includes some of the most highly sensitive parts in the limb, namely, those of the palm.

In such a case the retention of sensibility, although in an impaired degree, by the radial side of the palm, including pollex, is the cause, in our opinion, of the remarkable quantity and quality of movements still executed by the limb in spite of that impairment. In the case of section of the 7th, 8th, and 9th post-thoracic sensory roots, it was almost impossible, even on the closest examination, to detect any defect of movement whatever; the animal used both its feet apparently equally well in climbing or running, there was no clumsiness in picking up small objects (as a kernel) with the foot, and no hesitation in so doing. The chief detectable muscular difference between the limbs was that the knee-jerk was more brisk upon the operated side, and the calf and ham and back of the thigh were very deficient in sensation.

In the case of section of the sensory roots of the 7th and 8th cervical and 1st and 2nd thoracic, besides the ulnar border of the arm, the hand, with the exception of the thumb and radial side of index, is also quite insensitive; and the sensitiveness of the skin of the thumb and index, where present, is below normal in degree. Nevertheless, although impairment of movement (especially clumsiness) is obvious, yet the condition is strikingly different to that obtaining after the whole series of roots has been cut through. Food is easily taken up from the hand and picked up from the floor with the thumb and index of the partially apæsthete limb, and the limb is freely used in progression and in climbing. The grasp of the partially apæsthete hand is easier, however, to detach from the cage than that of the normal hand.

We find, again, that a degree of impairment very nearly as great, indeed not obviously different from that produced by section of the sensory roots of the whole series of the nerves of the limb, is produced by section of just those sensory roots which supply the apex of the limb, *i.e.*, the hand and foot. If the whole hand or foot be completely apæsthete by the sections, then the peculiar degree of inability to move the limb, described at the outset of this communication, is obtained in (as far as we can see) its full extent. This result has its converse in the following, which we have also noted:—

(3.) *Effect of Section of the whole Series of Sensory Roots belonging to a Limb, with the exception of a Root that supplies the Hand or Foot.* —If the sensory roots of the whole series of the spinal nerves belonging to a limb be severed, with the single exception of, in the upper limb, the 8th cervical (distributed to the whole extent of the hand), and in the lower limb the 6th post-thoracic (distributed to the whole extent of the foot), a certain degree of impairment of movement of the limb results, which appears rather as weakness than clumsiness, but the degree of impairment is altogether quite slight. The limb is used freely for progression, for climbing, for picking up food, and bringing it to the mouth. The grasp of the partially apæsthete hand is easier to detach from the cage than that of the hand of the normal side.

The question naturally arises whether in the interruption of the paths of afferent impulses these can be so dissociated as to decide what share the muscular sense takes in the results observed? The afferent nerve fibres from muscles *per se* run, in every case in which they have been examined, in the corresponding spinal roots to the motor. It is possible, therefore, in the hand and foot to sever the sensory roots supplying the muscles, while only partially interfering with those supplying other structures—the skin, sheaths of tendons, joints, &c. The converse is also true. On leaving intact the afferent fibres from muscle for the greater part, but interrupting all other afferent channels, we find that the defect of movement produced is nevertheless extreme, as, for instance, when the lowest four cervical and the first thoracic roots are severed. In the same way, when the 5th, 6th, and 7th post-thoracic roots are divided, the sole of the foot is apæsthete, and the defect in movement is extreme, although the afferent fibres from the plantar muscles remain to a large extent intact.

Conversely, when the 7th, 8th, and 9th post-thoracic roots are severed, the afferent fibres from the plantar muscles are completely interrupted, but the sole of the foot remains sensitive all over, owing to the 6th and 5th roots. In this case (and we have two instances of the kind), the defect of movement, if it exists at all, is not appreciable.

It must be remembered that other deeper structures are innervated by the same nerves as the skin, and these we do not wish to dissociate from the sensory channels still uninjured after interruption of all afferent tracts from the muscle itself.

II. *On Nutrition.*

In the experiments upon the lower limb we were at first led to suspect that section of the sensory roots caused trophic changes in the skin of the foot. After a time, varying from three weeks to three months, an ulcer appeared over the outer malleolus; the subsequent experiments on the upper limb, which never led to such a change, show, in our opinion, that the apparent trophic change in the lower limb may more justly be attributed to the liability to pressure and microbic infection. No change in the hand was ever noticed which in the least indicated trophic disturbance. Wounds accidentally inflicted by the animal itself or its companions on the apæsthete part healed readily when dressed.

As to the condition of the muscles in the apæsthete limb, which were themselves removed from all afferent connection with the central nervous system, the following points were noticed :—

There was a certain degree of wasting, but no appreciable alteration of colour; and the muscles responded readily to the excitation of their motor nerves. In some instances it was found that on excitation of their motor nerves, after somatic death, muscular contractions were evoked for a longer period than on the normal side. The time of onset of *rigor mortis* was delayed in the apæsthete muscles, as one of us has already noted in the cat.*

III. *Preliminary Note on the Degenerations observed.*

It is of interest to remark that the bulk, if not all, the fibres of Goll's column are derived from those sensory roots which contribute to the innervation of the lower limb. Certain of the sensory fibres innervating the limb enter, therefore, into the formation of a path leading directly to the cortex by the *posterior column nuclei*, the *fillet*, and the *optic thalamus*.

On the other hand, section of a short series of five dorsal and upper lumbar roots produced no appreciable degeneration in Goll's column. This appears to us a most suggestive fact, because the fibres from the limb thus entering Goll's column contribute to a path which leads *viâ* Goll's nucleus and the fillet to the opposite optic thalamus and cortex of the central convolutions. But the fibres from the roots above those entering the limb not contributing to Goll's column, their upward path must be *viâ* grey matter, and probably subsequently by one of the cerebellar tracts to the cerebellum.

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* C. S. Sherrington, 'Roy. Soc. Proc.,' vol. 53, p. 408.

In the cases of section of the brachial and upper thoracic posterior roots no degenerated fibres go into Goll's column, nor into Goll's nucleus, but there is very extensive degeneration of Burdach's nucleus. The upward path from the arm resembles therefore that of the leg in respect to its upward projection toward the cortex.

One of the chief results to which, in conclusion, we wish to draw attention is the following:—That afferent impulses, both from the skin and from the muscles, especially the former, as related to the palm and sole, are necessary for the carrying out of "highest level" movements.

It may be argued against this assertion that in animals it is impossible to obtain definite information as to whether the animal *can* not or *will* not perform the movements the absence of which has been described.

From observations on tame animals we are, however, ourselves firmly convinced that there exists actual inability to perform the movements in question. The reasons for this belief will be detailed in our fuller paper.

IX. "On the Development of the Branches of the Fifth Cranial Nerve in Man." By A. FRANCIS DIXON, B.A., M.B., Chief Demonstrator of Anatomy, Trinity College, Dublin. Communicated by Professor D. J. CUNNINGHAM, F.R.S. Received February 22, 1895.

(Abstract.)

The investigation, the general results of which are summarised below, was suggested to the author by Professor His, and part of the work was carried on in his laboratory in Leipzig in the summer of 1893. Models were constructed of the cranial nerves in embryos of different ages, and the branches present noted and measured. These models were made up of glass plates, covered with varnish, on which were drawn the outlines of the sections and the positions of the nerves, &c. Detailed descriptions of the fifth nerve branches are given for five different stages of the human embryo, beginning with an embryo of four weeks, at which time merely the three main divisions of the nerve are represented, and ending with one of the eighth week. The observations on the human embryo have been checked by further observations on rat embryos, and an almost complete correspondence between the two has been made out.

1. Before the ophthalmic nerve is present in the embryo, a cellular cord stretches upwards and forwards from the Gasserian ganglion, and occupies the place of the future nerve. This is easily demonstrated in the rat, and a similar condition appears to prevail in the human embryo.

2. Axis-cylinder processes grow out from the cells of the Gasserian ganglion into this cellular cord, and in this manner the ophthalmic nerve is formed.

3. The axis-cylinder processes increase in number, while the cells of the original cord become fewer and at the same time become disposed around the developing nerve.

4. The first formed ophthalmic trunk corresponds to the nasal nerve of the adult.

5. The frontal nerve is formed later, and the proximal part in man is united in a common trunk with the proximal part of the nasal nerve; in the rat, however, the nerves in the first instance take separate origin from the Gasserian ganglion.

6. In mammals no outlying part of the Gasserian ganglion is present as a ganglion, either for the ophthalmic or nasal nerve, in the sense of a ganglion of a posterior nerve-root.

7. A single undivided Gasserian ganglion gives rise to all three divisions of the fifth nerve.

8. In the beginning of the sixth week all the important branches of the ophthalmic nerve of the adult are represented in the embryo.

9. The fourth and frontal nerves are from an early period closely connected.

10. The ciliary ganglion is first recognisable as a distinct cellular mass at the beginning of the sixth week.

11. The ciliary ganglion appears, in the first instance, to be more closely connected with the frontal and fourth nerves than with the nasal and third nerves.

12. Later this ganglion shifts its position, and in the eighth week it has established the connections and assumed the situation that obtains in the adult.

13. The ciliary ganglion can in no sense be the homologue of a spinal ganglion.

14. The superior maxillary nerve appears as an independent branch of the Gasserian ganglion in the embryo of four weeks.

15. Soon the fibres of the superior maxillary nerve spread out horizontally, and are not collected so compactly into a single bundle as are the fibres of the other two divisions of the fifth nerve.

16. All the important branches of the superior maxillary are present in an embryo of seven weeks.

17. Meckel's ganglion is present as a distinct cellular mass at the beginning of the sixth week.

18. Meckel's ganglion in the embryo is closely connected with the otic ganglion.

19. The inferior maxillary nerve is at first unbranched.

20. The first formed part of the inferior maxillary represents the inferior dental of the adult.

21. The lingual nerve is formed later, and is still very small in the fifth week.

22. Meckel's cartilage presents a close relation to the branches of the inferior maxillary nerve, and seems to determine the directions which they take.

23. All the important branches of the inferior maxillary nerve are represented in the embryo at the beginning of the sixth week.

24. At the beginning of the sixth week the otic and submaxillary ganglia are present.

25. The chorda-tympani, the Vidian, and the nerve of Jacobson are not branches of the fifth nerve, inasmuch as they are not developed from it.

26. The chorda-tympani and Vidian nerves are branches of the facial, being developmentally derived from this nerve; the nerve of Jacobson is in like manner a branch of the glossopharyngeal.

27. The origin of these nerves in man is precisely similar to that of the corresponding nerves in the rat.

28. There is no direct evidence to prove that the cells of the accessory ganglia of the fifth nerve are derived directly from the cells of the Gasserian ganglion.

29. The sympathetic associated with the carotid artery in the adult is represented in the embryo by fibres chiefly derived from the trunk ganglion of the vagus nerve.

X. "Is Argon contained in Vegetable or Animal Substances?" By GEORGE W. MACDONALD, M.Sc., and ALEX. M. KELLAS, B.Sc. Communicated by Professor WILLIAM RAMSAY, F.R.S. Received March 19, 1895.

At Professor Ramsay's suggestion, experiments were undertaken to see whether argon could be obtained from nitrogenous vegetables or from animal tissues.

Method :—A few grams of the substance, after drying if necessary, were ground to a fine powder, desiccated at 110° C., until the weight was constant, and a nitrogen estimation performed by Dumas' method. It was supposed that any argon compound would be decomposed, when the argon would come off along with the nitrogen. The gas was collected in a nitrometer over well-boiled concentrated potash solution, whence it was transferred to a small gasholder containing water which had been boiled for some hours. The gasholder was connected with an apparatus similar in general arrangement to that used by Professor Ramsay in July last, which enabled the gas to be dried and purified from hydrocarbons, carbon monoxide or hydrogen, the nitrogen being absorbed by being passed and repassed over magnesium turnings kept at a temperature of about 600° C. A three-way stopcock enabled the magnesium tube and the purifying and drying tubes adjoining it to be connected either with the second gasholder or with a "Sprengel," so that any residual gas could be pumped off, and the tubes exhausted before admitting the gas. The gas pumped off was collected in a piece of hard glass tubing; it was then mixed with oxygen prepared by heating potassium permanganate, and sparked down until no further diminution of volume occurred, when excess of oxygen was known to be present. The remaining gas was transferred by a gas pipette to a tube standing over mercury, and the oxygen present absorbed by potassium pyrogallate solution. Only one experiment was carried through to the end in each case as the results seemed conclusive, although several determinations of nitrogen were made.

Peas were selected as a typical vegetable, and the following is a summary of the results:—

Expt.	Weight of desiccated peas taken.	Volume of nitrogen collected (corrected for temp. and press.).	Weight of nitrogen.	Percentage of nitrogen in dried peas.
	grams.	c.c.	gram.	
1	8·9446	465·8	0·5843	6·53
2	8·9455	466·8	0·5856	6·55

342 c.c. of the volume collected in the first experiment (which was done in two parts) was passed over the heated magnesium until so much nitrogen was absorbed that both gasholders were full of water, and the residue was then left for about 12 minutes in the tubes, the magnesium being kept heated so that absorption of nitrogen might continue. On connecting with the pump, a pressure of about 2 in. was registered, and the volume of gas pumped off had a volume of nearly 8 c.c. After sparking down with excess of oxygen 6·8 c.c. of gas was left, and on absorbing by a strong solution of potassium pyrogallate (stronger than Hempel recommends for complete absorption) 0·12 c.c. of gas remained, which is less than 0·04 per cent. of the volume taken. To see if this bubble of gas consisted of carbon monoxide, two experiments were performed with the oxygen

used: 9 c.c. gave 0·2 c.c. residue, and 11·2 c.c. gave 0·25 c.c. residue, with about the same strength of pyrogallate.

As the quantity of oxygen used in sparking down was over 20 c.c. it might safely be concluded that there is no appreciable quantity of argon in peas (or at least that the argon cannot be obtained with the nitrogen by Dumas' method).

The experiment with regard to the presence of argon in animal tissues was also negative in its results. Mice were selected for the experiment, because the nitrogen from the whole animal could be conveniently collected by Dumas' method. The crucial experiment was carried out in the same way as before.

From three mice 550 c.c. of nitrogen was obtained. This was absorbed by heated magnesium as usual, but the residual gas was left for over an hour in the tubes after the gasholders had been emptied, so that on connecting up with the pump only a few mm. of pressure was registered, and the total gas pumped off occupied about 1 c.c. After sparking down with large excess of oxygen about 10 c.c. remained, which gave a residue of 0·2 c.c. with alkaline pyrogallate, which would be about 0·036 per cent. of the volume taken.

Very careful experiments were undertaken to determine the exact percentage of nitrogen contained in mice, as the volume from the first experiments seemed too high. Two experiments with complete mice gave the following results:—

	Weight of mouse.	Percentage of water lost at 110° C.	Percentage of nitrogen in desiccated animal.
	grams.		
A	13·7	73·1	11·0
B	12·5	70·5	10·6

A Kjeldahl determination of the nitrogen in a small quantity of mouse A, by Mr. Frye, gave 9·6 per cent., so that the high percentage seems confirmed.

The Society then adjourned over the Easter Recess to Thursday, April 25.

Presents, March 21, 1895.

Transactions.

Berlin:—K. Preuss. Akademie der Wissenschaften. Sitzungsberichte. 1894. Nos. 39—53. 8vo. *Berlin.* The Academy.

Breslau:—Schlesische Gesellschaft für vaterländische Cultur. Jahres-Bericht. 71. 8vo. *Breslau* 1894. The Society.

Catania:—Accademia Gioenia di Scienze Naturali. Anno LXXI. 8vo. *Catania* 1894; Bullettino. Fasc. 36—38. 8vo. *Catania* 1894. The Academy.

Copenhagen:—Danish Biological Station. Report. 1893. 4to. *Copenhagen* 1894. The Station.

Cracow:—Académie des Sciences. Bulletin International. Février, 1895. 8vo. *Cracovie.* The Academy.

Halle:—Universität Halle-Wittenberg. Das zweihundertjährige Jubiläum: Festbericht. 4to. *Halle* 1895. The University.

Kazan:—Imperial University. Scientific Notes. [*Russian.*] 1895. No. 2. 8vo. *Kazan.* The University.

Kew:—Royal Gardens. Bulletin of Miscellaneous Information. No. 99. 8vo. *London.* 1895. The Director.

London:—British Astronomical Association. Memoirs. Vol. III. Parts 3, 4. 8vo. *London* 1895. The Association.

British Museum. Catalogue of Printed Books. Reg—Rem. Rev—Rib. Rib—Ric. Ris—Rob. Folio. *London* 1895. The Museum.

Camera Club. Journal. Vol. IX. No. 106. 8vo. *London* 1895. The Club.

London Mathematical Society. Proceedings. Vol. XXVI. Nos. 500—503. 8vo. *London* [1895]. The Society.

Physical Society. Proceedings. Vol. XIII. Part 4. 8vo. *London* 1895. The Society.

Lund:—Universitet. Års-skrift. Tom. XXX. 4to. *Lund* 1893–94. The University.

Manchester:—Manchester Literary and Philosophical Society. Memoirs and Proceedings. Vol. IX. No. 2. 8vo. *Manchester* [1895]. The Society.

Naples:—Società di Naturalisti. Bollettino. Vol. VII. Fasc. 3. Vol. VIII. Fasc. 1. 8vo. *Napoli* 1894–95. The Society.

Palermo:—Circolo Matematico. Rendiconti. Tomo IX. Fasc. 1—2. 8vo. *Palermo* 1895. The Society.

Paris:—École Normale Supérieure. Annales Scientifiques. Tome XII. No. 3. 4to. *Paris* 1895. The School.

Transactions (*continued*).

Stuttgart:—Königliches Staatsarchiv. Wirtembergisches Urkundenbuch. Bd. VI. 4to. *Stuttgart* 1894.
The Württemberg Government.

Observations and Reports.

Guatemala:—Dirección General de Estadística. Censo General de la Población de la República. 1893. 4to. *Guatemala* 1894.
The Direction.

Rio de Janeiro:—Observatorio. Annuario. 1894. 8vo. *Rio de Janeiro* 1893.
The Observatory.

Journals.

Agricultural Gazette of New South Wales. Vol. V. Part 12. 8vo. *Sydney* 1894.
Department of Agriculture, Sydney.

Boletin de Minas Industria y Construcciones. Año X. Num. 10. 4to. *Lima* 1894.
Escuela de Ingenieros, Lima.

Brioschi (F.) Notizie sulla Vita e sulle Opere di Arturo Cayley. 8vo. *Roma* 1895.
The Author.

Döllen (W.) Éphémérides des Étoiles pour la Détermination de l'Heure et de l'Azimuth au Moyen d'un Instrument des Passages Portatif établi dans le Plan Vertical de la Polaire. 1895. 8vo. *Dorpat.*
The Author.

Account of the appropriation of the sum of £4,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the Advancement of Science (continued from vol. lv, p. 305).

April 1, 1894, to March 31, 1895.

	£	*s.*	*d.*
A. M. W. Downing—for Continuation of the Computations for a New Edition of Taylor's Madras Star Catalogue	100	0	0
G. H. Darwin—for Payment of Computers in an Investigation of the Problem of the Three Bodies—Periodic Orbits	100	0	0
Carried forward	£200	0	0

	£	s.	d.
Brought forward	200	0	0
J. Holm and J. R. Erskine-Murray—for Experimental Investigations on Contact Electricity of Conductors ..	25	0	0
G. M. Minchin—for the Measurement of the Electromotive Forces developed in certain Photoelectric Cells by the Light of the Moon, the Planets, and the Fixed Stars, by means of improved appliances	50	0	0
O. Lodge—for assistance in experiments intended to elucidate the connection between Ether and Ordinary Matter..	150	0	0
Owen Glynne Jones—for the Determination of the Viscosity of Liquids by the measurement of the limiting speed of bodies falling through the liquid under the action of gravity	15	0	0
H. H. Turner (for Joint Permanent Eclipse Committee)—for Observations of the Total Solar Eclipse of August, 1893, in Norway and in Japan..............	250	0	0
P. L. Gray—for a Research on Radiation and especially on the Connection of Radiation with the Temperature of the Radiating Surface, &c......................	30	0	0
Prof. W. Ramsay—(1) for Continuation of Experiments on Molecular Surface Energy, especially on that of Fused Salts, Mixtures and Solutions (£25) ; (2) for Continuation of Researches on the ratio between the Specific Heats of Ethyl Oxide (£25)................	50	0	0
Prof. Dewar—for an Attempt to produce Liquid Hydrogen, and thereby study the Properties of Matter near the Zero of Absolute Temperature..............	400	0	0
Alfred E. Tutton—for the Purchase of a Goniometer to be used in a Research on the connection between the Atomic Weight of the Elements contained in the Crystallised Substance and the Goniometrical and Optical Constants	75	0	0
W. E. Wilson—for Apparatus in aid of further Research on the Radiation from Sun Spots and different parts of the Sun.................................	30	0	0
Dr. J. A. Harker—for Researches (1) on the Latent Heat of Steam ; (2) on the Specific Heat of Water, at temperatures from 0°—40°	30	0	0
The Incorporated Kew Committee (per C. Chree)—for a Research on the behaviour of Aneroid Barometers when exposed to a variety of Descending and Ascending Pressures	30	0	0
Carried forward........	£1,335	0	0

	£	s.	d.
Brought forward	1,335	0	0
C. F. Cross—for a Research on the Special Chemistry of the Celluloses, the Physiology of their Elaboration, and their Feeding Value	100	0	0
Dr. S. Schryver—for Investigation of the Oxidation Products of Turpentine Oil	20	0	0
W. H. Perkin, jun.—for (1) Continuance of Investigation on Camphoric, Camphoronic, and all Allied Acids; (2) an Investigation on the Synthetical Formation of Unsaturated Closed Carbon Chains	60	0	0
R. Threlfall—for Preparation of Pure Selenium, and Exact Determination of its Electrical Properties......	162	10	0
Dr. F. R. Japp—for an Investigation of the Reactions of Ketones, Diketones, and Allied Compounds........	75	0	0
J. N. Collie—for (1) the Preparation of various Oxy-Compounds of Pyridine, Picoline, and Lutidine; (2) the Preparation of the Corresponding Chlorine Derivatives; (3) the Preparation of the Acids obtained by the Oxidation of the Chlorides by means of Permanganate of Potash	20	0	0
W. A. Shenstone—for Continuing the Investigation of the Influence of Silent Discharge on Oxygen and other Gases in the purest state attainable............	30	0	0
W. P. Wynne—for Continuation of the Study of Quinoline Sulphonic Acids, and of certain Toluene Derivatives......................................	50	0	0
Prof. W. R. Dunstan—for a Continuation of the Investigation of the Aconite Alkaloids, both in respect of their Chemical Nature and Relationships, and of their Physiological Action................................	200	0	0
Prof. W. R. Dunstan—to Determine the Precise Conditions which are necessary for the "Rusting" of Iron	30	0	0
Dr. Percy Frankland—to Continue a Research on the Chemical Changes which are brought about in Pure Fermentation..................................	125	0	0
Dr. Forsyth-Major and Dr. Woodward—to enable Dr. Forsyth-Major to visit Madagascar, to investigate deposits yielding the Tertiary and Quaternary Faunas, and to secure specimens of living Vertebrata	300	0	0
R. Lydekker—for Continuation of the examination and description of Fossil Vertebrates of Argentina contained in the Museums of La Plata and Buenos Ayres..	130	0	0
Carried forward	£2,637	10	0

	£	s.	d
Brought forward	2,637	10	0
R. Irvine—for Assistance and Apparatus to be used in an Investigation of the Composition of Ocean Waters	50	0	0
Dr. John Murray—for (1) the Examination of Deep Sea Deposits received from various Localities; (2) a Research on the Pseudomorphic and other Changes induced by Metallic and Ammoniacal Salts in the Constituents of Marine Deposits	100	0	0
Dr. Thomas Johnson—for the Investigation of the Irish Marine Algæ; their Structure, Life-History, and Distribution	25	0	0
Cecil C. Duncan—for the Continuation of a Series of Observations on the Structure and Cause of the Movements of the Diatomaceæ	20	0	0
West India Committee (per G. Murray)—for carrying on the Work of the Committee, and particularly to send a Collector to Margarita	100	0	0
Sandwich Islands Committee (per D. Sharp)—to investigate the Fauna of the Sandwich Islands in continuation of the work of the Sandwich Islands Committee..	100	0	0
A. Willey—for an Expedition to the South Seas for the purpose of working out the Development and Life-History of the Pearly Nautilus	200	0	0
F. Galton (for a Committee)—for an Inquiry into the Measurable Characteristics of Plants and Animals	50	0	0
Prof. D'Arcy Thompson—to send a Collector to Jan Mayen's Land, Spitzbergen, and East Greenland, for the purpose of obtaining Zoological Specimens, and of making observations	40	0	0
Prof. T. W. Bridge—for further Researches in (*a*) Skeletal Anatomy of the Teleostean Fishes; (*b*) Morphology of the Skull in certain Ganoid Fishes	25	0	0
J. G. McKendrick—for a Research on the Functions of the Cochlea	50	0	0
Dr. Risien Russell—for a Comparative Investigation of Red and White Muscle	40	0	0
B. Moore—for a Research on the Effect of Removal of Certain Organs on "Stoffwechsel" in Dogs and Monkeys	75	0	0
W. D. Halliburton—for Continuation of Research on Nucleo Albumins and their Influence on Intravascular Coagulation	50	0	0
Carried forward	£3,562	10	0

	£	s.	d.
Brought forward	3,562	10	0
Dr. Leonard Hill—for the Continuation of a Research on Intra-cranial Pressure..............................	50	0	0
J. W. Washbourn—for an Experimental Inquiry upon the Nature of Immunity, especially with reference to the Pneumococcus..................................	20	0	0
V. Harley—for an Investigation of the Respiratory Gas Exchange in Diseases of the Respiratory and Circulatory System....................................	40	0	0
Rubert Boyce—for the Study of the Descending Degenerations in the Brain and Spinal Cord, and of the Seat of Origin and Paths of Conduction of the Motor Impulses..	100	0	0
C. A. Ballance and G. S. Shattock—for Continuation of the Research into the Pathology of Cancer........	40	0	0
Dr. A. E. Garrod—for further Researches on the Urinary Pigments................................	10	0	0
W. M. Bayliss—for a Research on the Mode of Action of the Vaso-motor Centre, with especial reference to Central Excitation of Vaso-dilators	40	0	0
S. M. Copeman—for a Study of the Pathology of Variola and Vaccinia..............................	40	0	0
E. H. Starling—for further Research on the Physiology of Lymph Formation	50	0	0
Dr. R. T. Hewlett—to prepare a Supply of "Tetanus Antitoxine," and to Investigate the Nature of the Substance or Substances which confer Immunity and which Neutralise the Tetanus Toxines......................	30	0	0
Dr. Sherrington—for Further researches on the Topography and Mode of Reflex and other Nervous Actions which employ the lower half of the Spinal Cord in the Monkey..	50	0	0
A. F. Stanley Kent—for a Study of the Changes occurring in the Nuclei and Protoplasm of Cells from various Animal Tissues as the result of experimentally produced injurious conditions	25	0	0
	£4,057	10	0

GENERAL FUND.

Dr.	£ s. d.		£ s. d.	Cr. £ s. d.
To Balance, March 31, 1894	515 4 9	By Appropriations, as above	4,057 10 0	
„ Parliamentary Grant	4,000 0 0	Less appropriations lapsed or abandoned	325 0 0	3,732 10 0
„ Repayments	230 5 9	„ Salaries, Printing, Postage, Advertising, and other Administrative Expenses		116 3 9
„ Interest on Deposit	13 8 6	„ Transferred to Reserve Fund		100 0 0
		„ Balance, March 31, 1895		810 5 3
	£4,758 19 0			£4,758 19 0

RESERVE FUND.

Dr.	£ s. d.		Cr. £ s. d.
To Balance, March 31, 1894	1,400 0 0	By Balance, March 31, 1895	1,500 0 0
„ Transfer from General Fund	100 0 0		
	£1,500 0 0		£1,500 0 0

Report of the Incorporated Kew Committee for the Year ending December 31, 1894.

The operations of The Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Incorporated Kew Committee, which is constituted as follows:—

Mr. F. Galton, *Chairman.*

Captain W. de W. Abney, C.B., R.E.
Prof. W. G. Adams.
Captain E. W. Creak, R.N.
Prof. G. C. Foster.
The Earl of Rosse, K.P.
Prof. A. W. Rücker.
Mr. R. H. Scott.
Mr. W. N. Shaw.
Lieutenant-General R. Strachey, C.S.I.
General J. T. Walker, C.B.
Rear-Admiral W. J. L. Wharton.

The Committee much regret the loss of the services of Admiral Sir G. H. Richards, K.C.B., formerly Hydrographer to the Admiralty, who has found it necessary to retire, after having served on the Committee from the date, 1871, when the Royal Society undertook the administration of the Observatory.

The vacancy thus occasioned has been filled by the appointment of Mr. W. N. Shaw, Tutor of Emmanuel College, Cambridge, and University Lecturer in Physics.

The work at the Observatory may be considered under the following heads:—

1st. Magnetic observations.
2nd. Meteorological observations.
3rd. Solar observations.
4th. Experimental, in connexion with any of the above departments.
5th. Verification of instruments.
6th. Rating of Watches and Marine Chronometers.
7th. Miscellaneous.

I. MAGNETIC OBSERVATIONS.

No change of importance has been made in the magnetographs during the past year. The curves representing the Declination, Horizontal Force, and Vertical Force variations have been obtained uninterruptedly, and, as in former years, the scale values of all the instruments were determined in January.

The ordinates of the various photographic curves were then found to be as follows:—

Declinometer: 1 inch = 0° 22′·04. 1 cm. = 0° 8′·7.

Bifilar, January 17, 1894, for 1 inch δH = 0·0280 foot grain unit.
„ 1 cm. „ = 0·00051 C.G.S. unit.
Balance, January 16, 1894, for 1 inch δV = 0·0287 foot grain unit.
„ 1 cm. „ = 0·00052 C.G.S. unit.

In the case of the balance magnetometer it was found necessary to re-adjust the instrument, and as at the same time its sensibility was slightly altered, the scale value was again determined with the following result:—

Balance, January 23, 1894, for 1 inch δV = 0·0276 foot grain unit.
„ 1 cm. „ = 0·00050 C.G.S. unit.

As regards magnetic disturbances, the most marked occurred on July 20 and August 20, though on the following dates the instruments were a good deal disturbed:—

January 3—4, February 21, 23—25, and 28, March 30—31, April 17—18, June 10, September 14—15, 19—20, and November 13.

An examination of the photographic curves of April 27 and July 10, made at the request of Mr. C. Davison, showed slight movements in the horizontal force magnet, and smaller ones in the declination magnet. These movements were associated by Mr. Davison ('B.A. Report' for 1894, p. 151, and 'Nature,' vol. 50, pp. 450—451) with the Greek and Constantinople earthquakes of the same dates. Similar examinations have since been made on one or two occasions at the instance of Mr. Davison, and of Professor Tacchini, Officio Centrale di Meteorologia, Rome, but with negative results.

The hourly means and diurnal range of the magnetic elements for 1894, for the quiet days selected by the Astronomer Royal, will be found in Appendix I.

The following are the mean results for the entire year:—

Mean Westerly Declination	17° 23′·0
Mean Horizontal Force............	0·18251 C.G.S. unit.
Mean Inclination	67° 26′·0
Mean Vertical Force..............	0·43914 C.G.S. unit.

A somewhat improved method has been adopted for standardising the curves from the vertical force magnetograph. This introduces no change in the tables of "Diurnal Ranges" in the case either of the Vertical Force or Inclination, but slightly affects the absolute values of the "Hourly Means." If the method previously in use had been followed, the mean results for the year would have been

Inclination..............	67° 25′·0
Vertical Force	0·43881 C.G.S. unit.

The observations of absolute declination, horizontal intensity, and inclination have been made weekly during the year.

A table of recent values of the magnetic elements at the Observatories whose publications are received at Kew was communicated to the July number of 'Science Progress,' and it is intended to contribute similar tables to the same magazine in future years. It is hoped in this way to render magnetic data more generally accessible to investigators than they have been hitherto.

Mr. Armitage, of the Jackson-Harmsworth Polar Expedition, was given a short course of instruction in the use of magnetic instruments immediately prior to the departure of the expedition in July. A unifilar magnetometer and a dip-circle were lent to the expedition on the condition of their being returned within a specified time.

Captain Lyons, R.E., practised for a few days the taking of magnetic observations previous to his departure for Egypt, and Captain Schück, of Hamburg, made a few comparisons of his instruments.

Mr. W. Watson, of the South Kensington School of Science, was, at the request of Professor Rücker, afforded facilities for making some comparisons of different magnetic instruments in the Experimental Magnetic House.

II. METEOROLOGICAL OBSERVATIONS.

The several self-recording instruments for the continuous registration of Atmospheric Pressure, Temperature of Air and Wet-bulb, Wind (direction and velocity), Bright Sunshine, and Rain, have been maintained in regular operation throughout the year, and the standard eye observations for the control of the automatic records duly registered.

The tabulations of the meteorological traces have been regularly made, and these, as well as copies of the eye observations, with notes of weather, cloud, and sunshine, have been transmitted, as usual, to the Meteorological Office.

With the sanction of the Meteorological Council, data have been supplied to the Council of the Royal Meteorological Society, the Institute of Mining Engineers, the editor of 'Symons's Monthly Meteorological Magazine,' Dr. Rowland, and others.

Anemograph.—Early in the year a new "worm" spindle was fitted to the direction fans, and the square-headed pricker was replaced by a round one, made of extra hardened steel. At the same time the direction pencil was "trued" in the lathe to improve the marking, and later on the velocity spiral was similarly treated.

Barograph.—The analysis of the value of the residual corrections mentioned in last Report showed that a re-determination of the barograph scale was expedient. This was carried out at the Meteorological Office, and showed that the old value of 1·569 inches in the curve ordinates to 1 inch of pressure should be replaced by 1·553 inches. The new value has been employed since January 1, and the irregularities of the residuals have been much less marked from that date.

Electrograph.—This instrument has been in regular action during the year, with the exception of about eleven days in January and nineteen days in August, and its general performance has been satisfactory. Advantage was taken of the frost in January to dismount the whole of the instrument, to remove the old acid in the jar and insulators, and give the apparatus a general overhauling.

At the same time the scale was slightly opened out. The suspension thread was accidentally broken on August 16, but it was replaced, and the instrument re-started on September 4.

Determinations of the scale value were made on March 30, June 26, and December 27 by direct comparison with the Portable Electrometer, White No. 53.

The value of the scale divisions of this latter instrument was kindly determined by Professor Carey Foster at University College Laboratory during February, and the value for one division found to lie between 197—205 volts. These experiments confirmed the scale value heretofore employed, viz., 1 scale division = 200 volts.

Inspections.—In compliance with the request of the Meteorological Council, the following Observatories and Anemograph Stations were visited and inspected:—Aberdeen and Glasgow, by Mr. Chree; Stonyhurst, Armagh, Fort William, Valencia, Deerness (Orkney), Fleetwood, and Dublin, by Mr. Baker; and Oxford, Falmouth, Alnwick Castle, North Shields, and Yarmouth, by Mr. Constable.

III. SOLAR OBSERVATIONS.

Sun-spots.—Sketches of Sun-spots have been made on 156 days, and the groups numbered, after Schwabe's method.

Particulars will be found in Appendix II, Table IV.

Time Signals.—These have been regularly received from Greenwich through the G.P.O., with the exception of a few days, on which occasions supplementary signals were transmitted at later hours.

IV. EXPERIMENTAL WORK.

Fog and Mist.—The observations of a series of distant objects, referred to in the last Report, have been continued. A note is taken of the most distant of the selected objects which is visible at each observation hour. An analysis of the results for the period May, 1892, to December, 1893, has been prepared and forwarded to the Meteorological Council.

Electrical Anemograph.—At the instance of the Meteorological Council the electrical anemograph referred to in the Report for 1890, with an improved arrangement for recording wind direction, has been under trial for some months.

Atmospheric Electricity.—To throw light on the results obtained with Lord Kelvin's water-dropper, a series of observations have recently been made for the Meteorological Office on the distribution of electric potential in the neighbourhood of the Observatory.

Aneroid Barometers.—A grant of £30 has been obtained from the Government Grant Committee for the purpose of conducting a research on the behaviour of aneroid barometers. The work of constructing the air pump and other apparatus required was entrusted to Mr. J. Hicks. He has unfortunately experienced considerable mechanical difficulties, which have delayed the construction of the apparatus. It is hoped, however, that it will be ready for use at an early date.

Thermometry.—Experiments are proceeding with a view to facilitate, and put on a more certain basis, the "time-test" in clinical thermometers—*i.e.*, the measurement of the time required by a thermometer to record the temperature of the body.

A sub-committee is now considering the question of the degree of accuracy to be aimed at in the comparison of thermometers, the form and construction of Kew Standards, and the advisability of supplying on the certificate forms fuller information than is given at present.

Lens Testing.—A large number of experiments have been made for the purpose of devising a photographic object for the definition test of lenses, which will supply more certain results than those hitherto obtained. A photographic object, reduced by Messrs. Morgan and Kidd, has given some fairly satisfactory results; but it is hoped a still further improvement may be effected by means of an object, suggested by Major Darwin, which is about to be constructed.

V. VERIFICATION OF INSTRUMENTS.

The subjoined is a list of the instruments examined in the year 1894, with the corresponding results for 1893:—

	Number tested in the year ending December 31.	
	1893.	1894.
Air-meters	15	4
Anemometers	24	2
Aneroids	59	48
Artificial horizons	15	31
Barometers, Marine	98	119
„ Standard	50	66
„ Station	30	12
Binoculars	466	417
Compasses	12	64
Deflectors	4	1
Hydrometers	591	289
Inclinometers	2	3
Photographic Lenses	31	27
Magnets	3	14
Navy Telescopes	913	249
Rain Gauges	19	6
Rain Measures	37	10
Sextants	517	461
Sextant Shades	47	0
Sunshine Recorders	1	1
Theodolites	2	4
Thermometers, Arctic	44	51
„ Avitreous or Immisch's	54	28
„ Chemical	57	64
„ Clinical	14,682	15,593
„ Deep sea	69	35
„ Meteorological	2,246	3,225
„ Mountain	18	23
„ Solar radiation	2	2
„ Standard	88	74
Unifilars	1	7
Vertical Force Instruments	0	6
Total	20,197	20,936

Duplicate copies of corrections have been supplied in 68 cases.

The number of instruments rejected on account of excessive error, or for other reasons, was as follows :—

Thermometers, clinical	91
„ ordinary meteorological	18
Sextants	60
Telescopes	17
Various	12

3 Standard Thermometers have been supplied during the year.

There were at the end of the year in the Observatory undergoing verification, 74 Barometers, 595 Thermometers, 9 Sextants, 3 Hydrometers, 2 Anemometers, and 3 Unifilar Magnetometers.

VI. RATING OF WATCHES AND CHRONOMETERS.

As was anticipated in last Report, the number of watches entered for the class B and C tests has been much reduced.

Those, however, entered for the higher test, class A, have been fully up to the average in number, and decidedly above the average in quality, the movements obtaining the highest grade of certificate, the class A, especially good, being considerably in excess of any previous year.

The 737 watches received were entered for trial as below :—

For class A, 378; class B, 183; class C, 166; and 10 for the subsidiary trial. Of these 7 passed the subsidiary test, 151 failed from various causes to gain any certificate; 115 were awarded class C certificates, 179 class B, and 285 class A; of the latter, 46 obtained the highest form of certificate, class A, *especially good.*

In Appendix III will be found a table giving the results of trial of the 46 watches which gained the highest number of marks during the year. The first place was taken by Messrs. Baume and Co., London, with a keyless, going-barrel, chronometer-watch, No. 103,025, with the "Tourbillon" escapement, which obtained 88·8 marks out of a maximum of 100.

The best performance of *lever* watches during the year was that of No. 52,882 by A. E. Fridlander, Coventry, which gained 87·3 marks.

The high position gained for several years past by Tourbillon watches has led to increased interest being taken by English manufacturers in this escapement, and some new forms and modifications have been devised.

Of these, one of the most successful at present appears to be that known as the "Karrusel." Specimens of this form have been sent for the A trial by different firms, and given excellent results, examples of which will be found in the list given in Appendix III.

Non-Magnetic Watches.—Fifteen watches thus designated have been examined during the year, both as to their ordinary time-keeping and also as to their "non-magnetic" properties, and although the trial to which they are submitted is severe, in the majority of cases the watches were found to perform very satisfactorily.

Marine Chronometers.—The second trial of chronometers on the Greenwich plan, mentioned in last Report, was finished in April. Of the 12 entered, 1 was withdrawn, and 8 of the remainder attained the limit prescribed by the Italian Government. A similar trial for the Portuguese Government was carried out from June to December.

A brief summary of the performance of the chronometers is given in Table III, Appendix III.

During the year 21 chronometers have been entered for the Kew A trial, of which 11 were certificated, 1 failed to pass, and 9 are still under examination.

VII. MISCELLANEOUS.

Lens Testing.—During the year 27 lenses have been tested; of these 7 received class A and 20 class B certificates. Some of the recent forms of Jena glass lenses have been under trial. With these there appears to be a superposition of two curvatures in opposite directions, one predominating near the centre, the other at the edge of the field. The resultant curvature is generally unusually small near the centre and over the greater portion of the field, but in some instances at least it increases rapidly near the edge of the field, and special attention should be paid to the size of stop to secure the best results.

Paper.—Prepared photographic paper has been procured and supplied to the Observatories at Aberdeen, Oxford, Stonyhurst, Lisbon, Mauritius, St. Petersburg, and through the Meteorological Office to Batavia, Fort William and Valencia.

Anemograph and rain sheets and sunshine cards have been supplied to the Hong Kong and Mauritius Observatories, and blank forms for the entry of magnetic observations to the Science and Art Department, London, the India Office, the Jackson-Harmsworth Polar Expedition, and Captain Lyons, R.E.

Exhibition of Cloud Photographs.—Some specimens of cloud and other photographs and lantern slides were shown by the Committee at the Royal Meteorological Society's Exhibition in April.

Pendulum Observations.—In December Mr. E. F. J. Love, of Melbourne, at the request of Mr. Ellery, was given the use of the sextant testing room for a few days for the purpose of swinging a set of half-second pendulums on the spot where observations were taken by Major von Sterneck in April, 1893.

House, Grounds, and Path.—The negotiations with Her Majesty's Office of Woods and Forests, referred to in last year's Report, have led to an increase of 5 acres in the area of the Old Deer Park leased by the Committee. The new lease contained the condition that the entire holding should be enclosed in a substantial fence. An oak park paling has accordingly been erected at a cost of rather more than £350. This expense, however, together with that of continuing the existing roadway from Fuller's Gate, through the new holding, has been wholly met by a gift of £400 made for the purpose by Mr. F. Galton.

Library.—During the year the library has received publications from—

34 Scientific Societies and Institutions of Great Britain and Ireland.

113 Foreign and Colonial Scientific Establishments, as well as from several private individuals.

Early in the year the library received from the Royal Society of Edinburgh a present of its Proceedings and Transactions from 1882, thus completing the series from 1867. These books, as well as several others, have been bound. The Königliche Preussische Akademie der Wissenschaften, Berlin, have kindly consented to forward periodically the mathematical and physical numbers of their Sitzungsberichte. The Meteorological Office presented several copies of meteorological publications of which they had duplicates. Lady Lefroy also presented some books belonging to the late Sir J. Henry Lefroy.

Back numbers of the Kew Reports have been sent to several institutions at the request of their respective directors, and some new names have been placed on the distribution list.

The card catalogue has been proceeded with, 200 cards having been entered during the past year.

Loan Repaid.—The Kew Committee have repaid the Royal Society the final instalment of the £400 advanced by them in 1893 to defray the cost of the new building.

Audit, &c.—An audit of the Observatory accounts for 1893 was carried out in May, 1894, by Mr. Keen, Chartered Accountant, on behalf of the Royal Society. In consequence of his suggestions, some changes have been introduced in the system of book-keeping.

The accounts of 1894 have been audited by Mr. Keen on behalf of the Royal Society, and by General J. T. Walker on behalf of the Committee.

The balance sheet, with a comparison of the expenditure for the two years 1893 and 1894, is appended.

PERSONAL ESTABLISHMENT.

The staff employed is as follows:—

C. Chree, M.A., Superintendent.
T. W. Baker, Chief Assistant.
E. G. Constable, Observations and Rating.
W. Hugo, Verification Department.
J. Foster " "
T. Gunter " "
W. J. Boxall " "
E. Dagwell, Observations and Rating.
R. S. Whipple, Accounts and Library, and six other Assistants.

A Caretaker and Housekeeper are also employed.

FRANCIS GALTON,
Chairman.

March 15, 1895.

Comparison of Expenditure (excluding Commissions) for the twelve months ending December 31st, 1893, and December 31st, 1894.

Net Expenditure.	1893.	1894.	Increase.	Decrease.
Administration—	£ *s.* *d.*	£ *s.* *d.*	£ *s.* *d.*	£ *s.* *d.*
Superintendent......	291 13 4	400 0 0	108 6 8	..
Office..............	148 18 0	94 10 0	..	54 8 0
Rent, fuel, lighting, &c..............	85 12 3	73 19 9	..	11 12 6
Attendance and contingencies........	219 3 1	210 6 8	..	8 16 5
"Whipple" Fund ..	50 0 0	..	..	50 0 0
Normal Observatory—				
Salaries............	340 18 5	334 10 6	..	6 7 11
Incidental expenses..	63 17 5	41 2 2	..	22 15 3
Researches—				
Salaries............	227 4 0	179 5 0	..	47 19 0
Incidental expenses..	..	0 10 0	0 10 0	..
Tests—				
Salaries............	866 18 0	868 14 9	1 16 9	..
Incidental expenses..	181 8 7	155 2 9	..	26 5 10
Normal expenditure, showing a decrease of £117 11s. 6d.	2,475 13 1	2,358 1 7	110 13 5	228 4 11
Royal Society—				
Repayment of Loan .	200 0 0	200 0 0	..	..
Construction of New Fence round Observatory............	..	304 3 6	304 3 6	..
Payment of Pendulum Account...........	117 1 7	..	..	117 1 7
Extension of Premises..	59 16 9	..	..	59 16 9
			414 16 11 405 3 3	405 3 3 ..
Total expenditure......	2,852 11 5	2,862 5 1	9 13 8	..

Kew Observatory. Account of Receipts and Payments for the year ending December 31st, 1894.

Dr. RECEIPTS.	£	s.	d.	£	s.	d.
To Balance from Year 1893				486	5	3
Royal Society:—						
Gassiot Trust. Annual payment	484	7	6			
" " Income tax returned	89	7	8			
				573	15	2
Meteorological Council:—						
Allowance	400	0	0			
Postages, &c.	5	4	3			
				405	4	3
Researches:—						
Meteorological Council	1	5	0			
Government Grant Committee	30	0	0			
				31	5	0
Tests:—						
Verifications	1310	9	8			
Rating	622	13	7			
Lenses	10	10	5			
				1943	13	8
Commissions executed for Colonial and Foreign Institutions, &c.				333	0	5
Extension Fund:—						
Mr. F. Galton's donation to cover cost of constructing Fence round Observatory				400	0	0
				£4173	3	9

PAYMENTS. *Cr.*	£	s.	d.	£	s.	d.
By Administration:—						
Superintendent	400	0	0			
Salaries	94	10	0			
Rent, Fuel, and Lighting	73	19	9			
Attendance, Cleaning, Repairs, and Insurance	210	6	8			
				778	16	5
Normal Observatory:—						
Salaries—Observations, Tabulations, &c.	334	10	6			
Incidental Expenses, Instruments, &c.	41	2	2			
				375	12	8
Researches:—						
Salaries—Observations, Reductions, &c.	179	5	0			
Incidental Expenses	0	10	0			
				179	15	0
Tests:—						
Salaries	868	14	9			
Incidental Expenses—Instruments, Postages, &c.	155	2	9			
				1023	17	6
Commissions for Colonial and Foreign Institutions, &c.				259	11	4
Royal Society:—						
Repayment of Loan for Extension of Premises				200	0	0
Extension Fund:—						
Payments on Account of Construction of Fence round Observatory				304	3	6
Balance:—						
Cash at the Bank of England (General Account)	829	15	2			
" " (Extension Fund Account)	95	16	6			
" London and County Bank, Richmond	112	10	0			
" Observatory (Petty Cash)	13	5	8			
				1051	7	4
				£4173	3	9

Audited and found correct.

On behalf of the Committee. (Signed) J. T. WALKER, *General.*

On behalf of the Royal Society. (Signed) W. B. KEEN, *Chartered Accountant*

January, 1895.

ESTIMATED ASSETS.

	£	s.	d.	£	s.	d.
By Balance as per Statement (General Account)	955	10	10			
,, ,, (Extension Fund Account)	95	16	6			
				1051	7	4
Payments due:—						
Meteorological Council—Allowance, Postages, &c.	114	9	7			
Test Fees	405	8	5			
Commissions	21	19	0			
				541	17	0
Stock:—						
Blank Forms and Certificates	50	2	8			
Standard Thermometers	87	14	0			
				137	16	8
				£1731	1	0

January 18th, 1895.

ESTIMATED LIABILITIES.

	£	s.	d.
To Administration accounts—Gas, Repairs, and Contingencies	23	17	1
Observatory accounts—A.G.B. Paper, Chemicals, &c.	10	4	9
Tests accounts—Fittings, Printing, &c.	11	18	2
Commissions	17	8	9
Fencing, &c. (Extension Fund)	74	0	0
General Balance	1593	12	3
	£1731	1	0

(Signed) CHARLES CHREE,
Superintendent.

List of Instruments, Apparatus, &c., the Property of the Kew Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Portable Transit Instrument................	1869
The Science and Art Department, South Kensington.	Articles specified in the list in the Annual Report for 1893..........................	1876
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete..............................	1883
	Pair 9-inch Dip-Needles with Bar Magnets ...	1887
Captain W. de W. Abney, F.R.S.	Mason's Hygrometer, by Jones	1885
Lord Rayleigh, F.R.S.	Standard Barometer (Adie, No. 655)	1885
R. J. Ellery, F.R.S. .	Pendulum Apparatus, complete, with Richard Thermograph..........................	1892
The "Jackson-Harmsworth" Polar Expedition.	Unifilar Magnetometer, by Jones, marked N.A.B.C., complete. Dip-Circle, by Barrow, with two Needles and Bar Magnets. Two Tripod Stands	1894

APPENDIX I.

MAGNETICAL OBSERVATIONS, 1894.

Made at the Kew Observatory, Richmond, Lat. 51° 28′ 6″ N. and Long. $0^{h}\ 1^{m}\ 15^{s}{\cdot}1$ W.

The results given in the following tables are deduced from the magnetograph curves which have been standardised by observations of deflection and vibration. These were made with the Collimator Magnet K.C. I. and the Declinometer Magnet marked K.O. 90 in the 9-inch Unifilar Magnetometer by Jones.

The Inclination was observed with the Inclinometer by Barrow, No. 33, and needles 1 and 2, which are 3½ inches in length.

The Declination and Force values given in Tables I to VIII are prepared in accordance with the suggestions made in the fifth report of the Committee of the British Association on comparing and reducing Magnetic Observations.

The following is a list of the days during the year 1894 which were selected by the Astronomer Royal, as suitable for the determination of the magnetic diurnal variations, and which have been employed in the preparation of the magnetic tables :—

January	9, 15, 19, 20, 27.
February	8, 10, 11, 14, 17.
March	5, 7, 13, 28, 29.
April..............	4, 11, 16, 22. 23.
May	6, 11, 12, 19, 27.
June..............	7, 13, 15, 26, 27.
July	7, 10, 14, 26, 31.
August............	2, 10, 17, 18, 28.
September	3, 4, 6, 13, 26.
October............	10, 11, 12, 23, 28.
November	4, 5, 12, 21, 22.
December..........	4, 11, 25, 26, 27.

Table I.—Hourly Means of Declination, as determined from the

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
(17° +) West						Winter.						
1894. Months.	′	′	′	′	′	′	′	′	′	′	′	′
Jan. ..	24·8	24·7	24·9	25·0	25·0	25·0	24·8	24·6	24·2	23·6	25·0	26·4
Feb. ..	22·2	22·0	22·6	22·9	23·3	23·7	23·7	23·4	22·7	22·4	23·7	25·9
March.	23·6	23·8	23·7	23·8	23·2	23·3	22·9	22·0	20·4	20·3	22·3	25·9
Oct. ..	19·3	20·0	20·4	20·2	20·1	20·1	19·8	19·8	18·7	18·8	20·4	23·0
Nov. ..	19·1	19·7	20·0	19·8	19·9	19·6	19·1	19·3	18·7	18·6	20·2	22·1
Dec. ..	19·2	19·4	19·4	19·4	19·4	19·2	19·0	18·9	18·8	18·5	19·5	20·6
Mean	21·4	21·6	21·8	21·8	21·8	21·8	21·6	21·3	20·6	20·4	21·9	24·0
						Summer.						
	′	′	′	′	′	′	′	′	′	′	′	′
April..	22·5	22·5	22·8	22·0	22·2	22·1	21·2	19·5	18·0	17·8	20·3	24·5
May ..	22·8	22·9	22·6	22·4	21·8	20·7	19·6	18·2	18·6	20·0	22·7	25·9
June..	22·7	22·5	22·1	21·3	20·7	19·4	18·1	17·4	18·0	19·3	21·4	24·1
July ..	22·2	21·9	21·7	22·1	21·5	20·0	19·1	19·1	18·7	19·7	22·2	24·7
Aug. ..	22·2	21·8	21·3	21·0	20·4	19·6	18·5	17·4	17·6	19·2	23·0	26·9
Sept...	19·9	19·9	19·9	20·1	19·7	19·6	19·0	18·0	18·1	19·6	22·3	25·8
Mean	22·0	21·9	21·7	21·5	21·1	20·2	19·3	18·3	18·2	19·3	22·0	25·3

Table II.—Solar Diurnal Range of the Kew

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
						Summer Mean.						
	′	′	′	′	′	′	′	′	′	′	′	′
	−1·1	−1·2	−1·4	−1·6	−2·0	−2·9	−3·8	−4·8	−4·9	−3·8	−1·1	+2·2
						Winter Mean.						
	′	′	′	′	′	′	′	′	′	′	′	′
	−1·6	−1·4	−1·2	−1·2	−1·2	−1·2	−1·4	−1·7	−2·4	−2·6	−1·1	+1·0
						Annual Mean.						
	′	′	′	′	′	′	′	′	′	′	′	′
	−1·3	−1·3	−1·3	−1·4	−1·6	−2·0	−2·6	−3·3	−3·6	−3·2	−1·1	+1·6

NOTE.—When the sign is + the magnet

selected quiet Days in 1894. (The Mean for the Year = 17° 23′·0 west.)

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid
Winter.												
′	′	′	′	′	′	′	′	′	′	′	′	′
28·4	29·7	30·0	29·0	28·0	27·6	27·3	26·8	26·2	25·8	25·5	25·3	25·
28·3	29·9	30·4	30·0	28·1	27·7	26·8	26·9	25·7	25·4	25·1	24·6	24·
29·4	30·9	30·7	29·0	26·9	25·7	25·3	25·2	24·8	24·6	24·4	24·0	23·
25·7	26·4	25·7	24·9	23·2	22·5	21·9	21·6	21·4	20·7	20·0	19·5	19·
23·5	24·8	24·3	22·9	22·0	21·3	20·9	20·5	20·1	19·7	19·5	19·7	19·
21·6	22·5	22·5	22·1	21·1	20·4	20·1	19·8	19·4	19·2	18·9	18·9	19·
26·2	27·4	27·3	26·3	24·9	24·2	23·7	23·5	22·9	22·6	22·2	22·0	21·
Summer.												
′	′	′	′	′	′	′	′	′	′	′	′	′
28·7	31·6	31·2	29·2	27·4	25·8	24·3	23·8	23·9	23·9	24·0	23·5	23
29·2	30·8	31·3	29·7	27·8	26·2	25·1	24·6	23·9	23·4	23·6	23·4	23
27·1	28·9	28·0	27·3	26·6	25·0	24·4	23·9	23·6	23·6	23·6	22·0	22
27·3	28·8	29·4	28·5	26·3	24·4	23·5	23·3	23·0	22·8	22·2	21·7	20
29·8	30·9	29·8	28·1	25·5	23·3	22·2	22·2	22·3	22·5	21·7	22·0	21
28·1	29·6	28·2	25·9	24·3	22·9	22·5	22·2	21·8	21·9	21·8	21·3	20
28·4	30·1	29·7	28·1	26·3	24·6	23·7	23·3	23·1	23·0	22·8	22·3	21

Declination as derived from Table I.

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mi
Summer Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+5·3	+7·0	+6·6	+5·0	+3·2	+1·5	+0·6	+0·2	0·0	−0·1	−0·3	−0·8	−1
Winter Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+3·2	+4·4	+4·3	+3·3	+1·9	+1·2	+0·7	+0·5	−0·1	−0·4	−0·8	−1·0	−1
Annual Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+4·3	+5·7	+5·5	+4·2	+2·6	+1·3	+0·6	+0·3	0·0	−0·2	−0·5	−0·9	−1

points to the west of its mean position.

Table III.—Hourly Means of the Horizontal Force in C.G.S. units (corrected

(The Mean for the

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
0·18000 +						Winter.						
1894. Months.												
Jan. ..	241	240	240	240	241	244	244	244	242	235	232	229
Feb. ..	237	236	236	237	240	245	246	247	244	236	227	224
March.	249	246	246	246	246	248	249	247	240	230	221	219
Oct. ..	258	257	258	260	259	263	264	260	254	244	235	232
Nov. ..	260	261	262	261	266	271	272	268	260	251	242	240
Dec. ..	264	265	263	263	265	266	266	265	263	260	254	254
Mean	252	251	251	251	253	256	257	255	251	243	234	233
						Summer.						
April..	256	255	255	254	255	257	256	251	238	226	217	213
May ..	264	263	259	257	257	257	253	244	236	228	226	225
June ..	260	259	260	259	258	257	250	243	235	226	221	225
July ..	255	254	252	254	252	250	242	241	234	223	217	219
Aug. ..	257	258	257	256	255	252	246	235	226	220	219	223
Sept. ..	258	255	254	253	253	253	248	242	230	221	219	224
Mean	258	257	256	256	255	254	249	243	233	224	220	222

Table IV.—Diurnal Range of the Kew

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
						Summer Mean.						
	+ ·00007	+ ·00006	+ ·00005	+ ·00005	+ ·00004	+ ·00003	− ·00002	− ·00008	− ·00018	− ·00027	− ·00031	− ·00029
						Winter Mean.						
	+ ·00001	·00000	·00000	·00000	+ ·00002	+ ·00005	+ ·00006	+ ·00004	·00000	− ·00008	− ·00017	− ·00018
						Annual Mean.						
	+ ·00004	+ ·00003	+ ·00003	+ ·00003	+ ·00003	+ ·00004	+ ·00002	− ·00002	− ·00009	− ·00017	− ·00024	− ·00023

Note.—When the sign is + the

for Temperature) as determined from the selected quiet Days in 1894.
Year = 0·18251.)

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid
Winter.												
229	233	235	236	239	244	247	250	248	249	248	247	24
227	233	237	243	243	247	249	252	252	251	251	250	24
225	234	240	245	249	247	250	250	251	250	250	252	25
239	246	253	255	255	261	264	264	263	265	264	264	26
240	244	251	254	259	264	267	268	266	266	266	266	26
254	258	261	262	266	269	271	270	270	266	265	266	26
236	241	246	249	252	255	258	259	258	258	257	258	25
Summer.												
217	224	232	242	247	255	257	260	260	258	257	256	25
229	238	247	259	268	278	278	277	276	274	271	269	26
234	244	252	262	271	273	277	277	273	271	269	264	26
223	229	242	254	257	265	266	268	265	263	263	262	25
231	238	248	254	258	261	264	266	264	262	260	260	25
232	243	244	246	248	252	258	261	263	263	263	262	26
228	236	244	253	258	264	267	268	267	265	264	262	26

Horizontal Force as deduced from Table III.

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid
Summer Mean.												
– 00023	– ·00015	– ·00007	+ ·00002	+ ·00007	+ ·00013	+ ·00016	+ ·00017	+ ·00016	+ ·00014	+ ·00013	+ ·00011	+ ·000
Winter Mean.												
– ·00015	– ·00010	– ·00005	– ·00002	+ ·00001	+ ·00004	+ ·00007	+ ·00008	+ ·00007	+ ·00007	+ ·00006	+ ·00007	+ ·000
Annual Mean.												
– ·00019	– ·00012	– ·00006	·00000	+ ·00004	+ ·00008	+ ·00011	+ ·00012	+ ·00011	+ ·00010	+ ·00010	+ ·00009	+ ·000

reading is above the mean.

Table V.—Hourly Means of the Kew Vertical Force in C.G.S. units (correcte
(The Mean for the

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
0·43000 +						Winter.						
1894. Months.												
Jan. ..	940	939	939	938	938	938	938	937	937	936	933	932
Feb. ..	927	926	926	926	926	925	926	925	926	927	923	921
March .	938	939	939	940	941	942	944	946	944	939	931	926
Oct. ..	917	917	915	915	915	914	914	915	915	912	905	902
Nov. ..	916	915	916	916	916	916	916	916	918	917	914	913
Dec. ..	910	910	910	910	910	909	909	909	910	909	909	910
Mean	925	924	924	924	924	924	925	925	925	923	919	917
						Summer.						
April ..	945	944	944	945	944	946	946	948	947	942	936	931
May ..	910	910	912	914	916	918	919	917	913	909	901	893
June ..	900	898	897	898	899	901	904	905	901	900	893	888
July ..	888	887	887	887	889	891	890	890	884	880	875	871
Aug. ..	888	888	888	887	889	892	892	892	889	884	879	877
Sept. ..	889	890	891	892	893	893	895	897	895	891	885	880
Mean	903	903	903	904	905	907	908	908	905	901	895	890

Table VI.—Diurnal Range of the Kew

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
						Summer Mean.						
	– ·00001	– ·00001	– ·00001	·00000	+ ·00001	+ ·00003	+ ·00004	+ ·00004	+ ·00001	– ·00003	– ·00009	– ·00014
						Winter Mean.						
	+ ·00001	·00000	·00000	·00000	·00000	·00000	+ ·00001	+ ·00001	+ ·00001	– ·00001	– ·00005	– ·00007
						Annual Mean.						
	·00000	·00000	·00000	·00000	+ ·00001	+ ·00002	+ ·00003	+ ·00003	+ ·00001	– ·00002	– ·00007	– ·00011

Note.—When the sign is + th

for Temperature), as determined from the selected quiet Days in 1894.
Year = 0·43914.)

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid
						Winter.						
932 920 925 901 914 910	933 923 931 905 916 911	937 927 936 908 919 913	940 931 942 911 920 914	940 932 945 914 920 915	938 931 944 915 919 913	938 929 942 914 918 912	938 927 941 912 917 911	937 927 940 913 917 912	937 926 939 915 918 912	936 925 939 915 920 912	935 926 939 914 919 913	93 92 93 91 92 91
917	920	923	926	928	927	926	924	924	925	925	924	92
						Summer.						
929 892 889 869 874 884	932 898 892 869 877 889	939 907 897 875 885 893	944 915 901 880 891 898	947 921 907 887 896 902	948 926 909 891 895 903	949 928 909 890 894 902	949 927 909 891 893 902	946 925 907 889 892 902	944 923 904 888 891 902	942 921 902 887 890 902	942 920 901 887 890 902	94 92 89 88 89 90
890	893	899	905	910	912	912	912	910	909	907	907	90

Vertical Force as deduced from Table V.

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid
						Summer Mean.						
− ·00014	− ·00011	− ·00005	+ ·00001	+ ·00006	+ ·00008	+ ·00008	+ ·00008	+ ·00006	+ ·00005	+ ·00003	+ ·00003	+ ·00
						Winter Mean.						
− ·00007	− ·00004	− ·00001	+ ·00002	+ ·00004	+ ·00003	+ ·00002	·00000	·00000	+ ·00001	+ ·00001	·00000	·000
						Annual Mean.						
− ·00011	− ·00007	− ·00003	+ ·00002	+ ·00005	+ ·00006	+ ·00005	+ ·00004	+ ·00003	+ ·00003	+ ·00002	+ ·00001	+ ·00

reading is above the mean.

Table VII.—Hourly Means of the Inclination, calculated from the Horizont

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
	67° +					Winter.						
1894. Months.	′	′	′	′	′	′	′	′	′	′	′	′
Jan....	27·3	27·3	27·3	27·3	27·2	27·1	27·1	27·0	27·2	27·6	27·7	27·9
Feb....	27·2	27·3	27·3	27·2	27·0	26·6	26·6	26·5	26·7	27·3	27·8	27·9
March.	26·7	26·9	26·9	27·0	27·0	26·9	26·9	27·1	27·5	28·0	28·4	28·4
Oct....	25·5	25·6	25·5	25·3	25·4	25·1	25·0	25·3	25·7	26·3	26·7	26·8
Nov. ..	25·4	25·3	25·2	25·3	25·0	24·6	24·6	24·8	25·4	26·0	26·5	26·6
Dec....	24·9	24·9	25·0	25·0	24·9	24·8	24·8	24·8	25·0	25·2	25·6	25·6
Mean	26·2	26·2	26·2	26·2	26·1	25·9	25·8	25·9	26·3	26·7	27·1	27·2
						Summer.						
	′	′	′	′	′	′	′	′	′	′	′	′
April..	26·4	26·5	26·5	26·6	26·5	26·4	26·5	26·9	27·7	28·4	28·8	28·9
May...	24·9	25·0	25·3	25·5	25·6	25·6	25·9	26·5	26·9	27·3	27·2	27·1
June ..	24·9	24·9	24·8	24·9	25·0	25·2	25·7	26·2	26·6	27·2	27·3	26·9
July...	24·9	25·0	25·1	25·0	25·2	25·3	25·9	25·9	26·2	26·8	27·1	26·8
Aug. ..	24·8	24·7	24·8	24·8	24·9	25·2	25·6	26·4	26·9	27·2	27·1	26·8
Sept. ..	24·8	25·0	25·1	25·2	25·2	25·2	25·6	26·0	26·8	27·3	27·3	26·8
Mean	25·1	25·2	25·3	25·3	25·4	25·5	25·9	26·3	26·9	27·4	27·5	27·2

Table VIII.—Diurnal Range of the

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
						Summer Mean.						
	′ −0·6	′ −0·5	′ −0·4	′ −0·4	′ −0·3	′ −0·2	′ +0·2	′ +0·6	′ +1·2	′ +1·7	′ +1·8	′ +1·5
						Winter Mean.						
	′ 0·0	′ 0·0	′ 0·0	′ 0·0	′ −0·1	′ −0·3	′ −0·4	′ −0·3	′ +0·1	′ +0·5	′ +0·9	′ +1·0
						Annual Mean.						
	′ −0·3	′ −0·2	′ −0·2	′ −0·2	′ −0·2	′ −0·2	′ −0·1	′ +0·2	′ +0·7	′ +1·1	′ +1·4	′ +1·3

NOTE.—When the sign is

and Vertical Forces (Tables III and IV). (The Mean for the Year = 67° 26′·0.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mi
						Winter.						
′	′	′	′	′	′	′	′	′	′	′	′	′
27·9	27·6	27·6	27·6	27·4	27·1	26·8	26·6	26·8	26·7	26·7	26·8	26·
27·7	27·4	27·2	26·9	27·0	26·7	26·5	26·2	26·2	26·2	26·2	26·3	26·
28·0	27·5	27·3	27·1	26·9	27·0	26·8	26·7	26·6	26·7	26·7	26·5	26·
26·4	26·0	25·6	25·6	25·7	25·3	25·0	25·0	25·1	25·0	25·1	25·0	25·
26·6	26·4	26·1	25·9	25·5	25·2	25·0	24·9	25·0	25·0	25·1	25·1	25·
25·6	25·4	25·2	25·2	24·9	24·7	24·5	24·6	24·6	24·9	24·9	24·9	24·
27·0	26·7	26·5	26·4	26·2	26·0	25·8	25·7	25·7	25·8	25·8	25·8	25·
						Summer.						
′	′	′	′	′	′	′	′	′	′	′	′	′
28·6	28·2	27·9	27·4	27·1	26·6	26·5	26·3	26·2	26·3	26·3	26·4	26·
26·8	26·3	26·0	25·4	25·0	24·4	24·5	24·5	24·6	24·6	24·8	24·9	25·
26·4	25·8	25·4	24·8	24·4	24·3	24·0	24·0	24·2	24·3	24·4	24·7	24·
26·5	26·1	25·4	24·8	24·8	24·3	24·2	24·1	24·3	24·4	24·4	24·4	24·
26·1	25·8	25·3	25·1	24·9	24·7	24·5	24·3	24·4	24·5	24·6	24·6	24·
26·4	25·8	25·8	25·8	25·8	25·5	25·1	24·9	24·8	24·8	24·8	24·8	24·
26·8	26·3	26·0	25·6	25·3	25·0	24·8	24·7	24·8	24·8	24·9	25·0	25·

Inclination as deduced from Table VII.

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid
						Summer Mean.						
′	′	′	′	′	′	′	′	′	′	′	′	′
+1·1	+0·6	+0·3	−0·1	−0·4	−0·7	−0·9	−1·0	−0·9	−0·9	−0·8	−0·7	−0·
						Winter Mean.						
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·8	+0·5	+0·3	+0·2	0·0	−0·2	−0·4	−0·5	−0·5	−0·4	−0·4	−0·4	−0·
						Annual Mean.						
′	′	′	′	′	′	′	′	′	′	′	′	′
+1·0	+0·6	+0·3	0·0	−0·2	−0·5	−0·7	−0·8	−0·7	−0·7	−0·6	−0·6	−0·

the reading is above the mean.

APPENDIX II.—Table I.

Mean Monthly Results of Temperature and Pressure for Kew Observatory.
1894.

Months.	Thermometer.								Barometer.*					Mean vapour-tension.
	Mean.	Means of—			Absolute Extremes.				Mean.	Absolute Extremes.				
		Max.	Min.	Max. and Min.	Max.	Date.	Min.	Date.		Max.	Date.	Min.	Date.	
1894.	°	°	°	°	°	d. h.	°	d. h.	ins.	ins.	d. h.	ins.	d. h.	in.
Jan.	38·9	42·8	34·2	38·5	51·3	11 NOON.	14·0	5 10 A.M.	29·883	30·587	3 10 A.M.	29·297	31 8 A.M.	·204
Feb.	41·9	47·3	36·4	41·9	55·0	7 1 P.M.	23·0	21 8 ,,	30·063	30·568	19 0.15 ,,	29·282	11 11 P.M.	·221
March..	44·2	52·3	36·7	44·5	63·0	31 2 ,,	29·1	19 3 ,,	29·984	30·497	23 10 ,,	28·996	13 6 A.M.	·228
April...	50·6	59·3	42·6	51·0	70·2	8 3 ,,	33·5	22 4 ,,	29·878	30·289	30 MIDT.	29·370	16 1 P.M.	·274
May ...	50·1	57·7	42·6	50·2	66·1	16 4 ,,	33·8	21 4 ,,	29·948	30·332	1 10 P.M.	29·558	{28 5 ,, {29 2 A.M.	·256
June ...	58·5	66·0	51·2	58·6	78·6	30 2&3 ,,	45·1	12 4 ,,	30·018	30·395	30 7 A.M.	29·681	6 8 P.M.	·364
July ...	62·1	69·9	54·8	62·4	83·4	6 4 ,,	48·7	14 4 ,,	29·898	30·320	1 0.5 ,,	29·284	11 2 A.M.	·418
Aug. ...	59·8	66·4	54·1	60·3	75·7	14 3 ,,	45·5	21 5 ,,	29·937	30·296	30 9 ,,	29·555	15 3 P.M.	·400
Sept....	54·2	60·5	48·4	54·5	67·6	1 3 ,,	36·0	28 6 ,,	30·137	30·507	30 11 P.M.	29·604	25 4 ,,	·335
Oct.....	50·1	55·1	45·2	50·2	61·7	11 2 ,,	32·6	17 7 ,,	29·924	30·518	1 9 A.M.	29·100	24 4 ,,	·313
Nov. ...	46·5	51·4	41·0	46·2	61·9	1 1 ,,	30·3	22 3 ,,	29·985	30·522	30 8 P.M.	28·809	12 7 ,,	·274
Dec. ...	42·1	46·1	37·3	41·7	51·6	14 3 A.M.	28·5	31 7 ,,	30·037	30·675	27 11 ,,	29·343	30 5 A.M.	·232
Yearly Means	49·9	56·2	43·7	50·0	..	....	..	....	29·974	..	....	..	....	·293

* Reduced to 32° at M.S.L. (The barometer cistern is 34 ft. above mean sea-level.)

This Table is compiled from "Hourly Means," vol. 1894, of the Meteorological Office.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall.*			Weather. Number of days on which were registered							Wind.† Number of days on which it was								
		Total.	Maximum.	Date.	Rain. ‡	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	Gales.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.
1894.		ins.	ins.																	
January	7·1	2·895	0·320	22	26	5	2	..	4	16	2	1	4	2	3	5	9	5	2	3
February	6·3	1·565	0·470	17	15	..	..	..	6	12	2	..	1	5	1	..	9	8	4	5
March	4·9	1·215	0·375	14	9	..	2	..	12	8	2	2	3	10	1	1	9	3	2	5
April	6·2	1·460	0·605	24	15	..	2	4	4	11	1	2	4	4	4	6	7	3	..	3
May	6·7	1·570	0·350	10	13	..	2	3	1	11	..	7	7	2	..	3	7	3	2	..
June	7·0	2·200	0·550	[illegible]3	12	..	..	1	5	17	..	4	3	2	..	1	12	4	4	..
July	6·8	4·370	1·435	29	17	..	..	3	3	13	..	1	2	4	..	5	10	7	2	3
August	6·8	2·525	0·625	24	17	..	..	2	3	13	..	5	2	2	..	1	10	7	4	7
September...	6·7	1·365	0·370	7	10	..	..	1	5	12	..	11	9	5	1	..	..	3	1	5
October	8·0	3·885	1·360	30	18	..	..	..	0	19	..	5	11	3	1	1	7	1	2	8
November...	6·5	2·980	0·585	14	15	..	..	1	3	10	1	2	4	..	1	10	8	2	3	5
December ...	7·2	1·990	0·880	14	16	..	..	..	3	15	1	2	2	1	1	4	11	4	6	6
Totals and means.	6·7	28·020			183	5	8	15	49	157	9	42	52	40	13	37	99	50	32	50

* Measured at 10 A.M. daily by gauge 1·75 feet above ground.

† As registered by the anemograph.

‡ The number of rainy days are those on which 0·01 inch rain or melted snow was recorded.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.				Maximum temperature in sun's rays. (Black bulb *in vacuo*.)			Minimum temperature on the ground.			Horizontal movement of the air.*		
	Total number of hours recorded.	Mean percentage of possible sunshine.	Greatest daily record.	Date.	Mean.	Highest.	Date. †	Mean.	Lowest.	Date. ‡	Average hourly velocity.	Greatest hourly velocity.	Date.
1894.	h. m.		h. m.		deg.	deg.		deg.	deg.		miles.	miles.	
January	53 24	20	7 0	26	67	86	20 29	29	11	5 6	13·9	42	4
February	72 48	26	8 0	12	76	103	27	31	13	22	14·0	40	11
March	161 24	44	10 0	26 31	97	122	31	30	21	18	11·9	36	13
April	145 48	35	11 18	10	109	129	1	36	26	22	9·5	37	16
May	171 0	35	14 24	24	116	129	30	37	23	22	11·9	29	24
June	165 30	34	15 0	30	119	136	21	47	38	12	10·0	29	2
July	173 42	35	14 18	1	124	138	8	50	41	8	9·2	27	7
August	142 48	32	9 54	20	119	134	7	48	36	17 21	9·5	31	25
September	97 30	26	10 0	11	100	120	6	43	30	29	7·8	28	9
October	50 12	15	5 48	29	85	106	26	41	25	17	9·0	32	24
November	72 42	27	6 48	18	80	105	1	35	25	29	11·0	41	14
December	44 24	18	6 6	15	64	81	23	31	23	1 & 31	11·0	40	22
Totals and Means	1351 12	29	..	..	96	..	..	38	..	..	10·7	..	..

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.

† Read at 10 A.M., and entered to previous day.

‡ Read at 10 A.M., and entered to same day.

Table IV.

Summary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days apparently without spots.
1894.			
January	12	16	—
February	15	17	—
March	15	11	—
April	19	10	—
May	11	16	—
June	12	17	—
July	13	17	—
August	15	14	—
September	13	12	—
October	10	11	—
November	11	16	—
December	10	12	—
Totals for 1894	156	169	—

APPENDIX III.—Table I.

Results of Watch Trials. Performance of the 46 Watches which obtained the highest number of marks during the year.

Watch deposited by	Number of watch.	Balance spring, escapement, &c.	Mean daily rate.					Mean variation of daily rate. ±	Mean change of rate for 1° F.	Difference between extreme gaining and losing rates.	Marks awarded for			Total Marks 0—100.
			Pendant up.	Pendant right.	Pendant left.	Dial up.	Dial down.				Daily variation of rate.	Change of rate with change of position.	Temperature compensation.	
			secs.	secs.	secs.	secs.	secs.	secs.	secs.	secs.				
Baume & Co., London	103025	Single overcoil, g.b., "tourbillon" chronometer	−1·5	−1·5	−1·4	−2·0	−1·5	0·4	0·05	3·5	32·6	39·3	16·9	88·8
Stauffer, Son, & Co., London	147625	Single overcoil, g.b., "tourbillon" chronometer	+0·4	−0·2	+0·8	+0·5	+1·2	0·4	0·04	4·0	32·8	38·5	17·1	88·4
A. E. Fridlander, Coventry	52882	Single overcoil, s.r., g.b., lever	+2·6	+2·8	+3·0	+3·0	+2·5	0·5	0·03	3·5	30·0	39·3	18·0	87·3
Stauffer, Son, & Co., London	147545	Single overcoil, g.b., "tourbillon" chronometer	+2·8	+3·3	+2·9	+1·0	+3·0	0·4	0·04	5·0	32·0	37·3	17·6	86·9
John Adams, Coventry	6162	Single overcoil, d.r., g.b., "Karrusel"	−3·0	−2·5	−2·8	+0·5	−2·9	0·3	0·04	4·7	33·4	35·8	17·5	86·7
A. E. Fridlander, Coventry	13977	Single overcoil, s.r., g.b.	+3·3	+1·2	+1·8	+1·5	+0·4	0·5	0·02	4·2	30·7	37·1	18·5	86·3
A. E. Fridlander, Coventry	14083	Single overcoil, s.r., g.b.	−1·1	+0·8	+3·4	−0·3	+2·8	0·3	0·03	6·2	33·5	33·7	18·1	85·4
A. E. Fridlander, Coventry	13758	Single overcoil, s.r., g.b.	+2·2	+1·2	+1·1	+1·2	+3·1	0·4	0·05	5·5	31·2	37·2	16·7	85·1
A. H. Ratliff, Coventry	66063	Single overcoil, s.r., g.b., "Karrusel"	+2·7	+2·1	+2·3	+1·3	+0·6	0·6	0·02	4·5	28·7	37·3	18·6	84·6
A. E. Fridlander, Coventry	14084	Single overcoil, s.r., g.b.	+2·3	+3·7	+0·8	+4·5	+3·2	0·5	0·03	5·2	30·7	35·7	17·7	84·1
A. E. Fridlander, Coventry	14006	Single overcoil, s.r., g.b.	+1·5	+1·7	+2·6	+1·5	+0·8	0·4	0·10	6·0	32·4	38·3	13·4	83·9
Jos. White & Son, Coventry	35142	Single overcoil, d.r., g.b., "tourbillon" (type I)	+0·6	+0·8	+0·4	+0·8	+3·8	0·6	0·02	5·7	29·0	36·0	18·8	83·8
John Adams, Coventry	6160	Single overcoil, s.r., g.b., "Karrusel"	−1·4	−1·8	−1·3	−1·9	+2·4	0·5	0·02	6·5	30·1	34·9	18·6	83·6
Usher & Cole, London	27980	Single overcoil, s.r., g.b., "Karrusel"	+2·0	+2·4	+2·4	+5·4	+0·9	0·4	0·05	5·7	31·1	35·6	16·5	83·2
H. Golay, London	102	Single overcoil, s.r., g.b., "Karrusel"	+6·5	+5·1	+5·6	+2·3	+5·0	0·5	0·05	9·5	30·9	35·8	16·5	83·2
John Adams, Coventry	6145	Single overcoil, s.r., g.b., "Karrusel"	+2·5	+2·4	+3·1	+1·1	+3·4	0·6	0·04	6·2	27·9	37·5	17·7	83·1
A. E. Fridlander, Coventry	14082	Single overcoil s.r., g.b. centre seconds	+0·6	+0·9	+1·1	+1·2	+2·5	0·6	0·06	7·2	28·7	38·1	16·2	83·0
W. Holland, Rockferry	3743	Single overcoil, s.r., g.b.	−0·5	+1·1	+0·1	−1·2	−3·0	0·5	0·04	5·7	29·2	35·6	17·6	82·4
Jos. White & Son, Coventry	34765	Single overcoil, s.r., g.b.	+1·3	−0·7	+0·6	+0·7	−0·7	0·6	0·04	7·7	28·3	37·0	17·1	82·4
A. E. Fridlander, Coventry	52884	Single overcoil, d.r., g.b.	+1·1	−0·5	−0·9	+2·7	+4·6	0·5	0·02	7·0	30·7	32·9	18·3	81·9
Usher & Cole, London	27990	Single overcoil, s.r., g.b.	+1·5	+5·3	+3·9	+3·0	+3·9	0·6	0·05	7·0	28·9	35·9	16·9	81·7
H. Golay, London	1991	Double overcoil, s.r., g.b.	+3·4	−0·5	+1·5	+2·5	+1·1	0·4	0·08	6·5	31·3	35·7	14·6	81·6

Table I—*continued.*

Watch deposited by	Number of watch.	Balance spring, escapement, &c.	Mean daily rate.					Mean variation of daily rate. ±	Mean change of rate for 1° F.	Difference between extreme gaining and losing rates.	Marks awarded for			Total Marks. 0—100.
			Pendant up.	Pendant right.	Pendant left.	Dial up.	Dial down.				Daily variation of rate.	Change of rate with change of position.	Temperature compensation.	
			secs.	secs.	secs.	secs.	secs.	secs.	secs.	secs.				
Jos. White & Son, Coventry	34595	Single overcoil, s.r., g.b.	+1·6	+4·1	+4·2	+1·9	+3·8	0·5	0·06	7·7	30·0	35·6	16·0	81·6
E. F. Ashley, London	04476	Single overcoil, s.r., fusee	−1·6	+1·3	−1·5	−1·9	−5·2	0·5	0·05	8·5	30·4	34·3	16·7	81·4
Jos. White & Son, Coventry	33449	Single overcoil, d.r., g.b.	+0·7	+0·1	−0·5	+0·4	−0·5	0·6	0·08	7·0	28·4	38·3	14·7	81·4
Jos. White & Son, Coventry	34948	Single overcoil, s.r., g.b., non-magnetic	−0·6	−1·3	−3·5	−0·8	+0·4	0·6	0·03	7·0	27·3	36·0	18·0	81·3
H. Golay, London	101	Single overcoil, s.r., g.b. "Karrusel"	+2·7	+2·5	+2·7	+1·6	+5·9	0·5	0·07	7·2	30·5	35·4	15·3	81·2
W. Holland, Rockferry	3756	Single overcoil, d.r., g.b. centre seconds	−0·9	−1·6	+3·2	+0·7	+1·2	0·6	0·01	7·7	27·6	34·3	19·3	81·2
Usher & Cole, London	27862	Single overcoil, s.r., g.b.	+1·7	+5·1	+2·6	+3·5	+0·3	0·5	0·04	6·5	29·1	34·7	17·3	81·1
A. E. Fridlander, Coventry	13716	Single overcoil, s.r., g.b.	−0·2	+0·1	+5·0	−1·6	−1·7	0·5	0·02	8·7	29·4	32·6	19·0	81·0
D. Buckney, London	11943	Double overcoil, d.r., g.b., centre seconds	−0·5	+3·5	+1·2	+3·4	+3·5	0·5	0·04	6·0	29·3	34·0	17·7	81·0
Jos. White & Son, Coventry	34152	Single overcoil, d.r., g.b.	+1·7	−0·9	+0·9	+1·7	+2·8	0·6	0·04	5·7	27·4	36·1	17·5	81·0
Jos. White & Son, Coventry	34076	Single overcoil, d.r., g.b.	+1·5	+0·7	+2·9	+1·7	+1·9	0·6	0·08	6·5	28·4	38·0	14·5	80·9
Jos. Player, Coventry	25833	Single overcoil (palladium), d.r., g.b.	−3·1	0·0	−0·5	+0·1	−1·6	0·6	0·04	6·0	27·4	35·8	17·7	80·9
W. Wordley, London	25363	Single overcoil, s.r., g.b.	−1·6	−0·1	+1·2	−2·5	−1·2	0·5	0·07	8·8	29·4	35·6	15·7	80·7
Baume & Co., London	3069	Single overcoil, d.r., g.b., chronograph	+2·9	+5·8	−2·0	+1·8	+2·5	0·6	0·01	9·7	29·0	32·6	19·0	80·6
Rotherhams, Coventry	95461	Single overcoil, s.r., g.b.	+0·1	−3·3	+3·2	+0·3	−1·0	0·5	0·06	7·7	30·8	33·6	16·1	80·5
H. Golay, London	4211	Double overcoil, d.r., g.b., minute repeater	+1·2	−0·2	+2·2	+2·2	+4·0	0·6	0·06	6·0	28·8	35·6	16·1	80·5
Baume & Co., London	3157	Single overcoil, d.r., g.b., chronograph	+4·1	+3·8	+1·3	+5·9	+6·1	0·5	0·06	6·7	29·9	34·4	16·1	80·4
Jos. White & Son, Coventry	34405	Single overcoil, d.r., g.b.	−1·7	+0·4	+0·9	+0·5	+1·0	0·6	0·06	6·7	27·6	36·9	15·9	80·4
H. Golay, London	1987	Double overcoil, s.r., g.b.	+2·5	+2·6	+0·6	+1·7	+2·4	0·5	0·12	6·8	30·8	37·4	12·1	80·3
A. E. Fridlander, Coventry	13848	Single overcoil, s.r., g.b. centre seconds	+1·2	+5·7	+4·3	+2·7	+2·6	0·5	0·06	7·5	29·9	34·6	15·8	80·3
Usher & Cole, London	27987	Single overcoil, s.r., g.b.	+2·3	+0·9	−0·4	+4·8	+3·6	0·5	0·04	7·7	29·6	33·6	17·1	80·3
Jos. White & Co. Coventry	34964	Single overcoil, s.r., g.b.	+0·5	+1·6	0·0	+1·5	+0·7	0·7	0·04	5·0	25·5	37·8	17·0	80·3
H. Golay, London	2153	Double overcoil, d.r., g.b. chronograph	+4·4	+3·5	+4·4	+2·8	+2·0	0·6	0·07	6·0	28·4	36·7	15·1	80·2
Jos. White & Son, Coventry	34065	Single overcoil, d.r., g.b.	+0·3	+2·7	+1·3	+1·0	+1·7	0·7	0·05	4·3	26·0	37·4	16·7	80·1

In the above List, the following abbreviations are used, viz.:—s.r. for single roller; d.r. for double roller; g.b. for going barrel; + for gaining rate; − for losing rate.

Table II.

Highest Marks obtained by Complicated Watches during the year.

Description of watch.	Number.	Received from.	Marks awarded for			Total marks, 0—100.
			Variation.	Position.	Temperature.	
Chronograph and perpetual calendar with moon's phases..............................	5858	S. Smith and Son, London....	31·8	31·4	16·3	79·5
„ „ „ and repeater	24968	„ „ ...	24·0	34·1	18·5	76·6
Minute and split seconds chronograph	3069	Baume and Co., London......	29·0	32·6	19·0	80·6
„ „ „	3157	„ „ „	29·9	34·4	16·1	80·4
„ „ „	153612	Stauffer, Son, and Co., London	26·8	37·2	14·8	78·8
Minute and seconds chronograph	2153	H. Golay, London	28·4	36·7	15·1	80·2
„ „ „	147412	Stauffer, Son, and Co., London	26·0	34·6	18·8	79·4
„ „ „	2135	H. Golay, London	28·3	32·6	13·7	74·6
Minute repeater..........................	4211	H. Golay, London	28·8	35·6	16·1	80·5
„ „	30820	D. Buckney, London	30·2	32·4	16·7	79·3
„ „	1958	H. Golay, London	27·8	35·4	15·5	78·7
"Non-magnetic" watches..................	34948	Jos. White and Son, Coventry	27·3	36·0	18·0	81·3
„ „	02122	S. Smith and Son, London....	29·2	32·0	18·0	79·2
„ „	02124	„ „ „	29·5	32·2	15·8	77·5

Table III.

Abstract of Performance of Chronometers on Trial for the Italian Government, from November, 1893, to April, 1894.

Name of maker.	Number of chronometer.	Whether 2-day or 8-day.	Description of balance, &c.	Least weekly sum.	Mean temperature for that week.	Greatest weekly sum.	Mean temperature for that week.	Difference between the greatest and least. (a.)	Greatest difference between one week and the next. (b.)	Mean temperatures for these two weeks.	Trial No. a + 2b.
				secs.		secs.		secs.	secs.		secs.
Johannsen, London	4363	2	Auxiliary, acting in heat....	− 4·7	55·1	+ 6·7	92·3	11·4	6·3	76·0—88·1	24·0
,, ,, ...	4385	2	,, ,, ...	− 6·5	45·4	+ 3·4	88·2	9·9	7·3	88·2—61·7	24·5
V. Kullberg, ,, ...	5435	2	,, bright spring	+ 0·2	45·4	+21·2	67·4	21·0	3·6	48·0—75·0	28·2
,, ,, ...	5385	2	,, ,, ...	− 9·7	47·1	+ 8·0	88·2	17·7	5·8	48·2—75·0	29·3
Johannsen ,, ...	4384	2	,, acting in heat....	− 8·0	75 0	+ 8·2	92·3	16·2	6·6	79·9—52·6	29·4
V. Kullberg, ,, ...	5418	2	,, bright spring	−11·5	45·4	+ 9·4	61·7	20·9	4·7	51·4—76 0	30·3
Johannsen ,, ...	4373	2	,, acting in heat....	−11·6	88·2	+ 3·1	52·2	14·7	9·9	88·2—61·7	34·5
Sewill ,, ...	4612	2	,, ...	−18·3	47·1	− 1·6	62·2	16·7	9·4	48·2—75·0	35·5

The extreme range of temperature was from 37°·8 to 103°·2 F.

Abstract of Performance of Chronometers on Trial for the Portuguese Government, from June to December, 1894.

Name of maker.	Number of chronometer.	Whether 2-day or 8-day.	Description of balance, &c.	Least weekly sum.	Mean temperature for that week.	Greatest weekly sum.	Mean temperature for that week.	Difference between the greatest and least. (a.)	Greatest difference between one week and the next. (b.)	Mean temperatures for these two weeks.	Trial No. a + 2b.
Kullberg, London	5452	2	Auxiliary, bright spring....	− 2·2	44·2	+ 9·5	94·7	11·7	4·2	86·6—49·1	20·1
,, ,, ...	5461	2	,, ,, ...	− 4·2	44·2	+ 6·5	86·6	10·7	5·0	86·6—49·1	20·7
,, ,, ...	5444	2	,, ,, ...	− 8·8	61·8	+ 6·5	86·6	15·3	2·8	75·3—80·7	20·9
,, ,, ...	5446	2	,, ,, ...	− 5·4	48·1	+ 5·7	94·7	11·1	6·1	75·0—67·7	23·3
,, ,, ...	5493	2	,, ,, ...	−15·3	{44·2 51·4	+ 2·1	70·0	17·4	4·0	94·5—75·0	25·4

The extreme range of temperature was from 35°·6 to 102·5° F.

Report of Magnetical Observations at Falmouth Observatory for the Year 1894. Latitude 50° 9′ 0″ N. and Longitude 5° 4′ 35″ W.; height, 167 feet above mean sea-level.

These observations have been made by instruments purchased from the Government Grant Fund administered by the Royal Society.

The Observatory having been comparatively recently established, the Vertical Force self-recording instrument is not yet in thorough working order. It is hoped in future to publish complete records of all three elements.

Photographic curves of Magnetic Declination and of Horizontal Force variations have been taken regularly throughout the past year, and the magnets have worked satisfactorily.

The scale values of the instruments were determined on 1st January, 1895. The following values of the ordinates of the photographic curves were then found:—

Declination, 1 cm. = 0° 11′·7.
Bifilar, for 1 cm. δ H., = 0·00050 C.G.S. unit.

The principal magnetic disturbances recorded during the year occurred on the following dates:—January 4, February 21, 23, 24, 25, March 30, 31, April 17, June 10, July 20, August 20, and September 14, 15.

Observations with the Absolute Instruments have been made monthly, of which the following is a summary:—

Determinations of Horizontal Intensity, 35.
" Inclination, 34 sets of four.
" absolute Declination, 36.

Following the method adopted in the three previous years, it is intended that the observations be reduced, and that the Declination and Horizontal Force curves for five quiet days in each month of the year—selected by the Astronomer Royal—be tabulated and prepared for publication, in accordance with the International scheme. The results will be printed in the Royal Cornwall Polytechnic Society's Annual Report, and also in the "Proceedings" of the Royal Society.

The following are the principal results of the magnetic elements for the year 1894:—

Mean Westerly Declination,	19° 0′·8.
Mean Inclination,	67° 2′·4.
Mean Horizontal Force,	0·18511 C.G.S. unit.

The Declination and Horizontal Force are deduced from hourly readings of the photographic curves, and so are corrected for the diurnal variation.

The Inclination is the mean of the absolute observations, the mean time of which is 3 P.M.

In Table V, X is the mean of the absolute values observed during the month (generally three in number), uncorrected for diurnal variations and for any disturbance. Y is the mean of the products of the Dips and X.

The results in the following tables, Nos. I, II, III, IV, are deduced from the magnetograph curves which have been standardised by observations of deflection and vibration. These were made with the Collimator Magnet marked 66A, and the Declinometer Magnet marked 66C in the Unifilar Magnetometer by Elliott Brothers, of London. Table No. V is deduced from these observations.

The temperature correction (which is probably very small) has not been applied owing to temporary change in the relation of the curve to the base line, caused by additions to the external gas-pipes.

The Inclination was observed with the Inclinometer No. 86, by Dover, of Charlton, Kent, and needles 1 and 2, which are 3½ ins. in length, the results of which appear in Table VI.

The Declination and Horizontal Force values given in Tables I to IV are prepared in accordance with the suggestions made in the fifth report of the Committee of the British Association on comparing and reducing magnetic observations, and the time given is Greenwich mean time, which is 20 min. 18 sec. earlier than local time.

The following is a list of the days during the year 1894 which were selected by the Astronomer Royal, as suitable for the determination of the magnetic diurnal variations, and which have been employed in the preparation of the magnetic tables:—

January	9, 15, 19, 20, 27.
February	8, 10, 11, 14, 17.
March	5, 7, 13, 28, 29.
April....	4, 11, 16, 22, 23.
May	6, 11, 12, 19, 27.
June	7, 13, 15, 26, 27.
July	7, 10, 14, 26, 31.
August............	2, 10, 17, 18, 28.
September	3, 4, 6, 13, 26.
October...........	10, 11, 12, 23, 28.
November..........	4, 5, 12, 21, 22.
December..........	4, 11, 25, 26, 27.

The whole of the instruments have been maintained in good order. The Magnetic Chamber and the Magnetic Hut in the garden have been kept in a satisfactory state of dryness during the year, save for six days in November, when the Chamber was flooded owing to excessive rainfall.

EDWARD KITTO,
Magnetic Observer.

Table I.—Hourly Means of Declination, at the Falmouth on five selected quiet Days in

(19° ± West.)

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
Winter.												
1894. Months.	′	′	′	′	′	′	′	′	′	′	′	′
Jan. ..	1·8	2·2	2·2	2·8	2·5	2·3	2·0	2·0	1·4	0·8	1·4	3·0
Feb. ..	0·6	0·6	0·6	1·3	1·3	1·5	1·5	1·2	0·7	0·3	0·9	2·7
*March .	2·6	3·2	3·4	3·4	3·0	3·0	2·6	1·5	0·0	−0·9	0·6	4·1
Oct. ..	−3·4	−2·5	−1·9	−2·2	−2·2	−2·4	−2·8	−2·7	−4·0	−3·6	−3·5	−0·6
†Nov. ..	−1·4	−1·1	−0·9	−0·9	−0·7	−1·5	−2·2	−2·0	−2·8	−3·5	−2·1	−0·2
Dec. ..	−3·4	−3·3	−3·3	−3·4	−3·3	−3·4	−3·7	−3·8	−3·9	−4·0	−3·5	−2·6
Means	−0·5	−0·2	0·0	0·2	0·1	−0·1	−0·4	−0·6	−1·4	−1·8	−1·0	1·1
Summer.												
	′	′	′	′	′	′	′	′	′	′	′	′
‡April ..	0·1	−0·2	−0·4	−0·7	−0·6	−0·4	−1·2	−2·5	−4·2	−5·2	−3·4	−0·1
May ..	0·2	−0·3	−0·5	−0·6	−1·1	−2·0	−3·3	−5·0	−5·2	−4·0	−0·9	1·3
June ..	0·6	0·5	0·0	−0·7	−1·1	−2·1	−3·3	−4·2	−4·2	−3·2	−1·5	0·5
July ..	0·1	−0·1	−0·1	0·1	0·0	−1·2	−2·8	−3·0	−3·9	−3·3	−1·2	1·3
§Aug. ..	0·0	−0·3	−0·4	−0·8	−1·3	−1·6	−3·1	−4·3	−4·9	−4·0	−0·6	3·2
Sept. ..	−2·8	−2·7	−2·4	−1·9	−2·1	−2·5	−3·0	−4·0	−4·5	−3·7	−1·4	1·9
Means	−0·3	−0·5	−0·6	−0·8	−1·0	−1·6	−2·8	−3·8	−4·5	−3·9	−1·5	1·4

* Mean of four days, 7th, 13th, 28th, and 29th. † Mean of four days, 4th, 5th, 21st, and 22

‡ Mean of four days, 11th, 16th, 22nd, and 23rd. § Mean of four days, 2nd, 10th, 17th, and 2

Table II.—Solar Diurnal Range of the Falmouth

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
Summer mean.												
	′	′	′	′	′	′	′	′	′	′	′	′
	−1·0	−1·2	−1·3	−1·5	−1·7	−2·3	−3·5	−4·5	−5·2	−4·6	−2·2	+0·7
Winter mean.												
	′	′	′	′	′	′	′	′	′	′	′	′
	−1·5	−1·2	−1·0	−0·8	−0·9	−1·1	−1·4	−1·6	−2·4	−2·8	−2·0	+0·1
Annual mean.												
	′	′	′	′	′	′	′	′	′	′	′	′
	−1·3	−1·2	−1·2	−1·2	−1·3	−1·7	−2·5	−3·1	−3·8	−3·7	−2·1	+0·4

Note.—When the sign is + the magnet

Observatory determined from the Magnetograph Curves each Month during the Year 1894.

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid.
Winter.												
′	′	′	′	′	′	′	′	′	′	′	′	′
5·1	6·6	7·2	6·3	5·1	4·6	4·2	3·9	3·4	3·0	2·4	2·4	2·3
5·3	7·5	8·2	8·1	6·3	5·6	4·5	4·5	3·9	3·1	2·7	2·2	1·9
7·6	10·2	10·4	8·9	6·8	5·2	4·4	4·2	3·6	3·6	3·2	2·7	2·2
2·6	4·1	3·9	2·9	1·0	−0·1	−0·4	−0·9	−0·9	−1·8	−2·0	−2·4	−2·4
1·8	3·3	3·1	1·7	0·9	0·4	−0·4	−0·6	−0·8	−1·6	−1·7	−1·5	−1·7
−1·1	−0·1	0·1	−0·4	−1·1	−1·8	−2·4	−2·6	−3·0	−3·2	−3·7	−3·7	−3·5
3·6	5·3	5·5	4·6	3·2	2·3	1·7	1·4	1·0	0·5	0·2	−0·1	−0·2
Summer.												
′	′	′	′	′	′	′	′	′	′	′	′	′
4·3	7·5	8·1	6·8	5·2	3·7	2·2	1·3	1·5	1·5	1·6	0·8	0·4
4·6	6·8	7·8	6·6	5·2	3·8	2·4	1·5	1·4	0·7	0·6	0·3	−0·3
4·3	6·2	5·8	5·5	5·2	3·4	2·6	1·6	1·6	1·5	1·5	0·4	0·2
4·3	5·8	7·1	6·7	5·0	3·1	2·0	1·4	1·2	0·8	0·2	0·3	−0·3
6·4	8·2	8·3	3·6	4·2	1·7	0·4	0·3	0·0	0·0	−0·6	−0·6	−0·2
5·5	7·2	6·3	4·3	2·4	0·6	0·1	−0·3	−0·7	−0·5	−0·6	−0·9	−1·4
4·9	7·0	7·2	5·6	4·5	2·7	1·6	1·0	0·8	0·7	0·5	0·1	−0·3

Declination as derived from Table I.

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid.
Summer mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+4·2	+6·3	+6·5	+4·9	+3·8	+2·0	+0·9	+0·3	+0·1	0·0	−0·2	−0·6	−1·0
Winter mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+2·6	+4·3	+4·5	+3·6	+2·2	+1·3	+0·7	+0·4	0·0	−0·5	−0·8	−1·1	−1·2
Annual mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+3·4	+5·3	+5·5	+4·3	+3·0	+1·7	+0·8	+0·4	+0·1	−0·3	−0·5	−0·9	−1·1

points to the west of its mean position.

Table III.—Hourly Means of the Horizontal Force at Falmouth on five selected quiet Days in each

0·18000 + (C.G.S. units.)

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
						Winter.						
1894. Months.												
Jan. ..	485	487	484	486	488	488	490	490	488	482	476	470
Feb. ..	499	496	496	497	500	504	505	508	506	499	489	482
*March .	516	513	512	513	514	515	516	514	507	495	487	483
Oct. ..	517	517	518	520	521	525	527	521	515	505	494	489
†Nov. ..	523	525	525	525	528	531	532	530	523	513	504	498
Dec. ..	520	520	519	518	520	522	522	521	519	515	511	506
Means	510	510	509	510	512	514	515	514	510	502	494	488
						Summer.						
‡April ..	512	511	511	510	511	511	512	505	496	481	472	466
May ..	533	530	526	524	524	524	521	512	504	495	490	488
June ..	531	530	532	529	528	527	519	511	502	499	490	489
July ..	518	517	515	516	515	513	507	504	496	485	478	479
Aug. ..	513	513	513	513	512	510	504	493	482	475	472	473
Sept. ..	523	520	519	518	518	517	514	508	498	487	480	484
Means	522	520	519	518	518	517	513	506	496	487	480	480

* Mean of four days, 7th, 13th, 28th, and 29th. † Mean of four days, 4th, 5th, 21st, and 22n

‡ Mean of four days, 11th, 16th, 22nd, and 23rd.

Table IV.—Diurnal Range of the Falmouth

(C.G.S. units.)

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
						Summer mean.						
	+ ·00009	+ ·00007	+ ·00006	+ ·00005	+ ·00005	+ ·00004	·00000	− ·00007	− ·00017	− ·00026	− ·00033	− ·00034
						Winter mean.						
	+ ·00001	+ ·00001	·00000	+ ·00001	+ ·00003	+ ·00005	+ ·00006	+ ·00005	+ ·00001	− ·00007	− ·00015	− ·00024
						Annual mean.						
	+ ·00005	+ ·00004	+ ·00003	+ ·00003	+ ·00004	+ ·00005	+ ·00003	− ·00001	− ·00008	− ·00017	− ·00024	− ·00027

Note.—When the sign is + the

Observatory as determined from the Magnetograph Curves,
Month during the Year 1894.

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid
						Winter.						
468	470	475	479	480	484	490	495	496	495	494	494	49
483	485	490	496	502	507	510	514	515	518	513	511	51
485	493	502	510	516	517	515	518	519	517	515	517	51
495	502	508	513	513	521	524	527	527	527	524	523	52
499	504	513	516	521	527	530	533	532	532	529	529	52
506	510	516	517	521	525	527	530	528	526	522	523	52
489	494	501	505	509	514	516	520	520	518	516	516	51
						Summer.						
467	475	483	496	503	511	517	521	520	518	514	513	51
490	498	509	519	534	544	544	541	541	543	538	535	53
498	505	513	531	545	544	545	549	545	543	542	536	53
483	488	499	512	521	529	529	529	529	527	523	525	51
481	491	499	508	515	519	523	524	523	520	517	515	51
493	504	507	510	511	516	522	526	528	528	527	526	52
485	494	502	513	522	527	530	532	531	530	527	525	52

Horizontal Force as deduced from Table III.

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid.
						Summer mean.						
− ·00028	− ·00019	− ·00011	·00000	+ ·00009	+ ·00014	+ ·00017	+ ·00019	+ ·00018	+ ·00017	+ ·00014	+ ·00012	+ ·00
						Winter mean.						
− ·00020	− ·00015	− ·00008	− ·00004	·00000	+ ·00005	+ ·00007	+ ·00011	+ ·00011	+ ·00009	+ ·00007	+ ·00007	+ ·00
						Annual mean.						
− ·00024	− ·00017	− ·00010	− ·00002	+ ·00005	+ ·00010	+ ·00012	+ ·00015	+ ·00015	+ ·00013	+ ·00011	+ ·00010	+ ·00

reading is above the mean.

Table V.—Magnetic Intensity. Falmouth Observatory, 1894.

1894.	C.G.S. measure.	
	X or Horizontal force.	Y or Vertical force.
January	0·18474	0·43642
February	0·18475	0·43814
March	0·18507	0·43688
April	0·18494	0·43647
May	0·18489	0·43603
June	0·18519	0·43660
July	0·18513	0·43684
August	0·18500	0·43643
September	0·18500	0·43657
October	0·18502	0·43662
November	0·18492	0·43660
December	0·18516	0·43618
Means	0·18498	0·43665

Table VI.—Observations of Magnetic Inclination. Falmouth Observatory, 1894.

Month.		Mean.	Month.		Mean.
		° ′			° ′
January	27	67 4·5	July	6	67 0·7
	29	67 3·1		27	67 2·2
	30	67 2·6		30	67 3·0
		67 3·4			67 2·0
February	21	67 9·9	August	13	67 2·4
	23	67 7·4		19	67 1·0
	24	67 7·2			
					67 1·7
		67 8·2			
			September	10	67 2·9
March	29	67 0·9		25	67 2·1
	30	67 1·3		29	67 1·2
	31	67 5·4			
					67 2·1
		67 2·5			
			October	8	67 1·6
April	28	67 2·6		19	67 3·3
	30	67 1·9		29	67 1·4
		67 2·2			67 2·1
May	9	67 2·1	November	8	67 3·9
	26	67 0·2		22	67 2·1
	30	67 1·6		29	67 2·0
		67 1·3			67 2·7
June	9	66 59·9	December	11	66 59·4
	21	67 2·2		21	67 0·6
	29	67 0·6		29	66 59·8
		67 0·9			66 59·9

OBITUARY NOTICES OF FELLOWS DECEASED.

ROBERT GRANT was born at Grantown, in Strathspey, on June 17, 1814, and received his early education at a school established by the Earl of Seafield, which provided, for the children of the district, instruction in those branches of education which are required to enter the Scottish Universities. But at the age of 13 his studies were interrupted by a serious illness; and it was not until the completion of his 19th year that his health was sufficiently restored to enable him to continue his studies with success. After a course of private study, Grant entered King's College, Aberdeen, but before the completion of the University course he removed to London and became engaged in business pursuits under his brother. At this time, Grant commenced to collect materials for a history of physical astronomy, and in 1845, in order to obtain access to further sources of information, he removed to Paris, where he supported himself by teaching English. In Paris, Grant had the great advantages of attending the astronomical lectures of Arago and Le Verrier, and the use of the books in the library of the Institute, of which he availed himself in the collection of the materials for his history.

Grant returned to London in 1847, and devoted himself to the preparation of his History of Physical Astronomy. This work was first published in parts in 'The Library of Useful Knowledge:' the first part appeared in September, 1848, and, although the author was previously unknown to most of the leading English astronomers, it immediately attracted attention, and led to Mr. Grant being elected a Fellow of the Royal Astronomical Society. The history was published in a separate form in March, 1852; and in the following November Grant was elected a member of the Council of the Royal Astronomical Society, and was appointed editor of 'The Monthly Notices;' and in 1856 the Gold Medal of the Society was awarded to him for his work. Grant's History of Physical Astronomy had certainly no rival in the English language at the time of its publication; it can still be consulted with great advantage upon any of the points with which it treats, and its impartiality is unquestionable.

In November, 1859, Grant was appointed Regius Professor of Astronomy and Director of the Observatory in the University of

Glasgow, and after a short course of practical instruction at Greenwich, he took charge of the Observatory at Glasgow, and continued to hold that post until the time of his death in 1892.

The Observatory was provided with an Ertel Transit Circle of six French inches aperture, and the chief astronomical work of the Observatory during Professor Grant's tenure of office was done with this instrument. The staff at the Glasgow Observatory was not a large one, and the salaries attached to the assistantships were so small that Grant had great difficulty in retaining for any length of time the services of skilled assistants, but he was able, by great perseverance and personal devotion to the work, to complete a catalogue of 6415 stars, reduced to the epoch 1870, and a catalogue of 2156 stars for the epoch 1890. The last proof sheet of the second catalogue was only passed by him on the morning of his death. Many of the stars in these two catalogues have not been recently observed at any other observatory, and the places are, therefore, comparatively of greater value than would otherwise be the case, and the results have already been found valuable for very closely approximate determinations of the proper motions of many stars not before known, and for the reduction of comet observations.

The results, however, can hardly be regarded as strictly independent determinations. For reasons which appeared sufficient to Grant it was thought expedient to compare the places of the stars observed at Glasgow with existing recent observations at Greenwich of the same stars, and to deduce corrections for the Glasgow places observed since 1865 to render them in mean results identical with the Greenwich results. The corrections thus found were then applied to the places of the Glasgow catalogue, which should, therefore, not differ systematically from those which would have been obtained had these stars been observed at Greenwich. There are, of course, advantages in such a method of proceeding when the observations have been made with an instrument of a class not suitable for independent work, but much is lost by the want of independence of the results if the instrument is a reliable one. It is, however, impossible for anyone not well acquainted with the Glasgow instrument to judge of the necessity for the course pursued by Grant in this case.

Besides the observations made with the Transit Circle, many extrameridional observations were made with the 9-inch equatorial by Cooke, the public clocks of Glasgow were controlled from the Observatory, and regular photographic meteorological observations maintained.

Professor Grant took part in the Solar Eclipse expedition to Spain in 1860. He was elected a Fellow of the Royal Society in 1865.

Professor Grant married September 3, 1874, Elizabeth Emma, daughter of A. C. Davison, of Newcastle, New South Wales, by whom he leaves one son and three daughters. He died at Grantown on October 24, 1892, in the 79th year of his age.

With Admiral Smyth and Professor Baden-Powell, he published a translation of Arago's 'Popular Astronomy,' two vols., 1855—1858, and a volume of 'Biographical Notices of Scientific Men.' He greatly contributed to the exposure of the Pascal forgeries (see 'Comptes Rendus,' 1867), by calling attention to the data extracted from Newton's published works and used by him, which were quoted in the forged letters, but which were not in existence in Pascal's lifetime; he also communicated seventeen papers to the Royal Astronomical Society.

E. J. S.

Arthur Milnes Marshall was born at Birmingham on June 8, 1852, and died on December 31, 1893. He was the second son of William P. Marshall, for many years Secretary of the Institution of Civil Engineers. He entered St. John's College, Cambridge, in 1871, and read for the Natural Sciences Tripos. In those days the Biological Sciences did not hold the prominent position in the University curriculum which they do at the present time, but the influence which has since brought about such a marked development of these studies at Cambridge had already begun its work, and Marshall was one of the first to take advantage of the new state of things.

As an undergraduate his studies met with every success: he was elected Scholar of his College in June, 1873, and in 1874 he was placed at the head of the first class of the Natural Sciences Tripos. After taking his degree in January, 1875, he went to Naples, having been appointed by the University to their table at the recently-established Zoological Station. He returned in the summer of 1875, and in the October Term joined with Balfour in giving a course of lectures, accompanied by practical work, on Zoology. He was elected Fellow of St. John's College in 1877.

In the same year he gave up—temporarily as it fortunately turned out—the prosecution of his purely scientific studies, and went to St. Bartholomew's Hospital to work at medicine. His medical studies continued until 1879, when he was appointed to the newly-established chair of Zoology in the Owens College.

He was now able to devote himself wholly to the pursuit of science. He took up again the embryological work which he had begun at Naples, and which had necessarily somewhat languished during his medical studies, and published a series of memoirs in the 'Quarterly Journal of Microscopical Science' on the morphology of the vertebrate head. These were entitled: "The Development of the Cranial Nerves in the Chick," 1878; "The Morphology of the Vertebrate Olfactory Organ," 1879; "Observations on the Cranial

Nerves of Scyllium," 1881 (in conjunction with his pupil W. B. Spencer); "On the Head Cavities and Associated Nerves of Elasmobranchs," 1881; to which must be added "The Segmental Value of the Cranial Nerves," published in 1882 in the 'Journal of Anatomy and Physiology.' These memoirs all influenced in an important manner the views and work of the cranial morphologists of the time.

But the principal part of his work at Manchester was connected with the development of the zoological teaching, and of the Museum; and as Secretary, and later, Chairman of the General Board of Studies, with the organisation of the courses of study of Victoria University. These duties left him but little time for research, but in spite of this drawback we have in the latter years of his life the following works from his pen:—

"On the Nervous System of *Antedon rosaceus*," 1884.

"Report on the Pennatulida, dredged by H.M.S. 'Triton,' and H.M.S. 'Porcupine,'" published in the 'Transactions of the Royal Society of Edinburgh,' vols. 32 and 33.

"On the Development of the Kidneys and Fat Bodies in the Frog," and "On the Development of the Blood Vessels in the Frog," both in 1890; and in conjunction with his pupil, E. J. Bles.

Although his multifarious duties at Owens left him but little time for research, he was fully alive to its importance; and he encouraged it in his pupils, and gave them every opportunity of prosecuting work of the kind, as witnesses of which statement are the two volumes of Studies from the Biological Laboratory of the Owens College, published in 1886 and 1890.

As a teacher, he was excellent—clear and accurate. A good measure of his capacities in this direction may be obtained by an examination of the three text-books which he wrote: "The Frog," the "Practical Zoology," and the "Vertebrate Embryology." Accuracy, clearness, and thoroughness in their various grades are the most characteristic features of these books.

He was a man of great energy and determination of character, of a kindly and cheerful disposition which made him very popular with his friends and colleagues; and of an enthusiastic nature, which contributed largely to his success as a teacher.

His death, which took place when he was climbing Scawfell with some friends, was due to an accident. Apparently the stone on which he was standing slipped from him, and he fell on to the rocks below

His loss will be felt not only at Owens, but all over the North of England, where he was well known by his persistent advocacy of the extension movement, to further which he gave much of his own valuable time.

He was elected a Fellow of the Royal Society in 1885, and served on the Council in the Year 1891–92. He was President of Section D, at the Leeds meeting of the British Association (1890), and was for many years President of the Manchester Microscopical Society. He was D.Sc. of London University (1877) and M.D. of Cambridge (1882).

A. S.

CHARLES ROMLEY ALDER WRIGHT, who became a Fellow of the Royal Society in 1881, was born at Southend, in Essex, in 1844. His father, Romley Wright, a civil engineer, was at one time employed on the Ordnance Survey of this country, and in the course of his work made some observations on the geology of the county of Salop, which were published in the 'Transactions of the Geological Society' for 1837. During the son's boyhood his parents removed to Manchester, and in due time Charles Wright was entered at Owens College to be educated for the profession of his father. At that period—the early years of the 60's—the college was housed in Quay Street, in the dingy, old-fashioned, red-brick building which had served Richard Cobden as a residence. In the rear of the building there had been erected a well-equipped chemical laboratory—indeed, as things went at that time, it was held to be a very paragon among such places—and here young Wright, under the stimulating guidance of Roscoe and Schorlemmer, found ample opportunity to develop the liking for experimental chemistry which had manifested itself during his home life. Quick-witted, bright and intelligent, with a capacity for boyish fun, and a fertility of resource in the exercise of it, which excited the admiration and at times, it must be added, the amazement of his less-gifted fellows, Wright appeared to overtake his college work with an ease and celerity that seemed to them little short of marvellous. Thanks, however, to his father's training, he had been early schooled into the habit of doing with all his might whatsoever his hand found to do, and this capacity for work never left him. His career at Owens was exceptionally brilliant; prize after prize, not only in chemistry, but also in physics, which he studied under Clifton, in mathematics, which he had from Sandeman, and in classics, which he learnt from Greenwood, fell, session after session, to his share. His course in connexion with his graduation work at the University of London was hardly less distinguished. On matriculating, in 1863, he gained the highest place and prize in chemistry; and at the intermediate examination in the following year he was awarded the Exhibitions both in chemistry and natural philosophy, and was made an Associate of Owens College.

Wright's first essay in experimental investigation was done under Roscoe's direction. It consisted of an enquiry into the relative sensitiveness of photographic paper salted with different haloids and mixtures of haloids, and was published in the 'Journal of the Chemical Society' for 1866. All his tastes predisposed him towards an academic position, but circumstances compelled him to seek for employment in technology, and shortly after his first paper appeared he accepted a situation as chemist at the Weston Works of the Runcorn Soap and Alkali Company. Here, with characteristic ardour, he at once began a number of investigations relating to the manufacture of sulphuric acid and alkali, the main results of which appeared in the 'Journal of the Chemical Society' and in the 'Chemical News.' He soon, however, turned his face towards London, and became an assistant to the late Dr. Bernays, in the laboratory attached to St. Thomas's Hospital. Afterwards he joined Matthiessen, and the two chemists began the remarkable series of investigations on the opium alkaloids with which their names, as joint authors, are associated, and one outcome of which was the discovery of apomorphine, one of the most powerful and valuable emetics known. In 1869 he was associated with Sir Lowthian Bell in an elaborate investigation on the theory of iron smelting, the results of which are contained in Sir Lowthian Bell's well-known work 'On the Principles of the Manufacture of Iron and Steel.'

In 1871 Wright became Lecturer on Chemistry at St. Mary's Hospital, and he occupied this position at the time of his death. He once more turned his attention to the chemistry of the alkaloids, and began the study of the essential oils, and during the next ten years memoir followed memoir in quick succession. Either alone or in conjunction with his pupils he contributed nine papers on Derivatives of Morphine and Codeine to the Royal Society (1871–74); six papers on Isomeric Terpenes to the 'British Association Reports' (1873–77); six papers on the Action of Organic Acids and their Anhydrides on Natural Alkaloids (1874–80); seven papers on Narceine, Narcotine, Cotarnine, and Hydrocotarnine to the Chemical Society (1874–77); four papers on the Alkaloids of the Aconites (1877–79); and three on the Alkaloids of the Veratrums (1878–79) to the 'British Association Reports,' in addition to occasional papers on other departments of vegetal chemistry. During the latter portion of this period he also contributed a series of Reports on Chemical Dynamics to the Chemical Society (1878–80). Questions relating to chemical physics had always great interest for him; and he sent to the Physical Society, of which he was one of the original members, a series of nine papers on the Determination of Chemical Affinity in terms of Electromotive Force, and, in conjunction with Professor Roberts-Austen, he made a number of measurements of the Specific Heat of Hydrogenium (1873). For some years back he occupied himself with the study of alloys, and the Proceedings of the Royal Society contain a number of papers by him on this subject. Indeed, his last paper, written a few days before his death, on the invitation of the London Section of the Society of Chemical Industry, consists of a *résumé* of the conclusions to which his long-continued work on this subject had led him. He maintained his touch with technology until the last, and he was an acknowledged authority on those branches of manufacturing chemistry with which he had become practically acquainted during his career. Gifted with a remarkable literary faculty and power of exposition, Wright was much sought after as a contributor to the literature of chemical technology. He wrote a number of articles on tar distilling and tar products in 'Muspratt's Dictionary;' the articles on iron and steel in the last edition of the 'Encyclopædia Britannica,' and the monographs on "Soap," "Sulphur," and "Sulphuric Acid" in Thorpe's 'Dictionary of Applied Chemistry.' In 1878 he gave a series of lectures at the Royal Institution on "Metals and their Industrial Applications," which were subsequently expanded and published in book form; and, in 1885, he gave a course of Cantor lectures at the Society of Arts on the "Manufacture of Soap." He was a frequent contributor to the proceedings of the London Section of the Society of Chemical Industry, and the Journal of the Society contains a number of his communications.

Wright took an active share in the formation of the Association which ultimately developed into the Institute of Chemistry, and he was a member of the Council of that body.

Much of his work was done under the depressing conditions of indifferent health, and at times he experienced great physical pain. From his boyhood he had suffered from an affection of the hip joint, which eventually ended in lameness, and the consequent inability to take exercise reacted very markedly upon his general health. His friends, however, were hardly prepared for the suddenness of his end. After an illness of a few hours' duration, he died on the 25th July, 1894, having barely attained his forty-ninth year.

T. E. T.

GEORGE JOHN ROMANES was born at Kingston, Canada West, on May 20, 1848; but, his parents having soon after removed to England, his childhood was spent in London or its immediate neighbourhood. Ill-health having rendered it desirable that he should reside in a warmer climate, the family subsequently left England, and, in consequence, his education was somewhat irregular. The surroundings of his boyhood and youth were, however, both enjoyable and healthful, and conduced to the development of that love of out-of-door life and intense interest in the external aspects of nature, which were so prominent in his after life. Romanes entered at Caius College, Cambridge, in 1869, and subsequently obtained a scholarship in that college. He graduated through the Natural Science Tripos in 1870. After taking his degree he entered on the study of medicine, and threw himself into the work of the hospital and dissecting room with great ardour; but in 1872 he was completely disabled by a severe and protracted illness, which led to his abandoning his intention of entering the profession of medicine. The writer became acquainted with him in the autumn of 1874. He had occupied himself during the previous year in biological studies, and was above all an ardent disciple of Mr. Darwin, with whom he had already corresponded, and soon after came into personal relation. For a couple of years Romanes was a worker in the Physiological Laboratory at University College, London, and dated therefrom (1876) his first contribution to the Royal Society. In the autumn of the same year he began his physiological experiments on Medusæ and sea urchins, and erected for the purpose a convenient marine laboratory close to his house at Dunskaith, at the entrance of Cromarty Firth, where he spent many happy autumns, engaged partly in pursuit of game, partly in serious work. He first became known to the general public in 1878, when he was invited to deliver one of the evening lectures at the British Association at Dublin on "Animal Intelligence," the substance of which took the form of a book in 1881. This was followed by a volume on 'Mental Evolution in Animals,' and, later, by 'Mental Evolution in Man.' He was admitted to the Fellowship of the Royal Society in 1879. In 1885 he undertook to deliver an academical course in Edinburgh, which comprised some thirty lectures, and extended over three years. He was subsequently (1888) appointed Fullerian Professor at the Royal Institution, and brought together the substance of his London and Edinburgh lectures in a treatise entitled 'Darwin and after Darwin.' His other works will be referred to in the following paragraphs.

Romanes died suddenly of cerebral hæmorrhage on May 23, 1894. Threatening symptoms of arterial disease had appeared in the summer of 1892, which were followed by a formidable illness in July, 1893. After this he travelled to the south of France, returning to Oxford, where he had resided since 1890, in the spring in much improved health. Up to the end he preserved not only his mental vigour, but the keenness of his interest in his scientific pursuits.

It is proposed in the following paragraphs first, to give an account of Romanes' physiological work, then of his contributions to what is now understood by the term biology, and, finally, to refer very briefly to his psychological writings.

The subject of his first communication to the Royal Society (May, 1876) was "The Influence of Injury on the Excitability of Motor Nerves." Of this paper, it is sufficient to say that the observations were made with great care, and that the new facts recorded have been fully confirmed by later observers.

The first of the very extended series of researches relating to the "locomotor system" of the Medusæ, starfishes, and sea urchins was published in the 'Philosophical Transactions' the same year, 1876, the last in 1881. These are so well known in consequence of their having been placed before the public in one of the volumes of the International Scientific Series, that it is sufficient to notice one or two points of interest relating to them. As regards the Medusæ, the chief value of Romanes' numerous and varied experiments consisted in the light they threw on elementary questions in physiology relating to excitability and conduction. As regards the arrangement and distribution of the nervous system, it is interesting to note how closely his inferences from experiment coincided with the anatomical facts brought to light a year or two later by the researches of the brothers Hertwig. In the researches on the Echinodermata, which were undertaken five years later, Romanes was aided in the anatomical part of his inquiry by his friend Professor Ewart. Throughout, his mode of working was the same, and was characteristic of the worker. His method was not to elaborate a plan of research beforehand, ascertaining what questions had been left uninvestigated by previous observers, but rather to find out all he could, before he began to experiment, about the phenomena which presented themselves to his observation in the out-of-door study of animal life. Residing, as has been said, at the entrance of Cromarty Firth, the animals that he chose as the subjects of inquiry were those which most abounded in its waters. In the study of starfishes and sea urchins, he began by observing their mode of progression, the way in which the animals recover themselves when placed in unnatural positions, the way in which they encounter or escape from an attacking enemy, and then proceeded by the ordinary methods of physiological experiment to the investigation of the functions of the nervous system, and to the determination of the way in which the movements of the animal are co-ordinated. Throughout, the value of his results was enhanced by the circumstance that he availed himself freely of the opportunity which his position afforded him of multiplying his experiments, and confirming every observation by repeating it on individuals belonging to a variety of species.

Between 1882 and 1893 Romanes offered no further contribution of any moment to the Royal Society; but in the latter year (see 'Proceedings,' vol. 54, p. 333) he submitted summaries of two experimental inquiries, one of which, on Plant Excitability, was so closely related to his early physiological studies that it may be most conveniently noticed here. Its purpose was to prove that the bending of plants towards any source of light is so little dependent on the quan-

tity and so entirely on the *suddenness* of the illumination, that a series of instantaneous exposures at intervals of two seconds produces in a given time a greater effect than continuous illumination from the same source for the whole period—a proof that in this case the action in question is "liberated," not "maintained," by light. The remarkable capacity for experimental investigation which was exhibited in these and in his earlier physiological studies made it a matter of regret to many of his friends that he did not continue them.

An account must now be given of his work in the field of general biology. His interest in Darwinian questions dates from the time which preceded and followed his becoming acquainted with Mr. Darwin in 1874. This acquaintance soon ripened into a friendship, to the reality of which the writer can testify. It was promoted first by the fact that in those early student days, Romanes' mind was, so to speak, steeped in Darwinian doctrine; but it owed its permanence to his possessing certain rare and brilliant intellectual qualities which Mr. Darwin highly appreciated.

As he himself tells us (see 'Nature,' vol. 8, p. 432, 1873), it was in discussing with Mr. Darwin some of the cases in which the effects of disuse appear to be inherited, that his attention was directed to a principle—now held by many biologists to be of great importance—that of the action of "panmixia" or "negative selection" (Weismann), or as Romanes at that time called it, "cessation of selection." His claim to priority in this matter was, however, weakened by the circumstance that he did not set forth this principle as of universal application—as the *only* way by which, consistently with the recognition of the "all sufficiency of natural selection" (Weismann, 'Cont. Review,' p. 334), the disappearance of superfluous organs can be accounted for. Another subject of interest to him was also of directly Darwinian origin. It seems to have been suggested by a well-known passage in which Mr. Darwin pointed out that specific characters which "do not affect the welfare of the species" cannot be attributed to natural selection ('Origin,' 6th ed., p. 176). Romanes' ideas on this point did not take form until, in 1886, he communicated to the Linnean Society his well-known article on "Physiological Selection; an additional Suggestion on the Origin of Species." The title was perhaps an unfortunate one, for it suggested a purpose which was very far from being that of the writer. It seemed like an attack on the fundamental doctrine of the 'Origin.' Notwithstanding its rather speculative character, the essay on "Physiological Selection" must be considered as the most important of his biological writings, for it sets before the reader, in terms which cannot be mistaken, that if by species is meant an association of animals or plants which is distinguished by the incapacity of its members to unite sexually with outsiders, the coming into existence of this disability as a barrier between them is a factor in the origin of species of which sufficient account has not been taken.

Romanes' precise position in relation to this contentious subject may be stated very shortly. Other naturalists had shown that isolation of part of a species, whether by geographical or other external obstacles to sexual intercourse, inevitably led to the acquirement by the plants or animals thus separated, of characters different from those of the remainder, and eventually to the setting up of the bar of reciprocal sterility between them. To this, without professing to explain it, Romanes added the statement that the setting up of this "physiological" barrier *may be the primary event*, *i.e.*, that it may come into existence between one part of a species and the rest in the absence of any structural difference, or other external obstacle to sexual union, and thus, having no cause itself, become the prime cause of all other differences. He himself admitted that his "theory" was but a statement of what he believed actually to happen, leaving the explanation of it, with that of variation in general, to the "future of physiology" ('Linn. Soc. Zool. Proc.,' vol. 19, p. 337). During the following half-dozen years he spent much time and labour in bringing together confirmatory instances, the results of which will no doubt soon be published. The chief claim to consideration of this theory consists in the prominence which it gives to what Romanes calls "mutual sterility, with fertility *inter se*," as that which constitutes the essence of species; in other words, to the subordination of all other specific distinctions to the one primary and dominant one —*sterility to outsiders.* Had he contented himself with showing that whenever a variety is promoted to the rank of a species, that promotion consists in the setting up of the "bar of sterility"—that the protection of this barrier, however set up, is essential to the progressive modification by natural selection of the species—and that there are reasons for believing that the functional disability may come into existence in the absence of recognisable structural difference, no one would have fallen into the mistake of supposing that it was his intention to substitute a new doctrine for the Darwinian.

As regards the question whether acquired characters are inherited, it is sufficient to say that both the direct and the indirect evidence seemed to Romanes to tend towards the affirmative. His physiological training probably made him more alive than he would otherwise have been to the difficulties which present themselves in all those cases in which the apparatus required for the performance of a complicated function must be in complete working order before it can be of the slightest use to the organism, and consequently must have arrived at perfection of development independently of the operation of natural selection. The particular case in which he emphasises this difficulty is that of the complicated automatic and perfectly co-ordinated bodily movements of the higher animals, the performance of which involves the co-operation of a complicated system of sense organs, sensory nerves, centres, motor nerves, and muscles; it being obvious that the power of so co-ordinating the actions of these various parts as to ensure the accomplishment of the required purpose is as certainly transmitted to offspring by inheritance as any morphological character. All that Romanes has written on this subject—voluminous as it is—is in complete accord with Mr. Herbert Spencer's contention, that the "coadaptation of parts cannot be accounted for by the natural selection of variations arising fortuitously." This doctrine he did his best to corroborate by new examples, considering Mr. Spencer's line of argument exceedingly "cogent against all ultra-Darwinians."

In dealing with the much more difficult question of the direct evidence for or against the inheritance of acquired characters, Romanes found the same difficulties as other naturalists. It is well known that for many years he sought, with praiseworthy perseverance, to verify the experimental results obtained by the late Dr. Brown-Séquard as regards the transmission to offspring of certain structural defects and pathological tendencies, originally produced in guinea-pigs by injuries of the central nervous system. Although some of the experiments made for this purpose yielded interesting results, the appearances observed were not sufficiently constant or decisive to enable him to confirm Brown-Séquard's conclusions. In addition to these experiments of his own, he was unceasing in his efforts to bring together all the experimental evidence that he could get hold of, either by reading or correspondence, relating to the conditions under which structural modifications produced by environment persist in the offspring of plants and animals for many generations, independently of the external conditions which originally produced them. On the question of so-called Telegony, he endeavoured to obtain information, partly by correspondence, partly by experiments (see 'Contemporary Review,' 1893, p. 512). To what extent these efforts were productive, the writer is unable to say, but there seems reason to hope that the collected evidence, when published in the forthcoming second volume of 'Darwin, and after Darwin,' will be of considerable interest. It may be noted that, although Romanes agrees with Mr. Spencer in regarding telegony, if true, to be fatal to the doctrine of "Germplasm," his idea of its mechanism is different.

During the year which preceded his death, Romanes published the work entitled 'An Examination of Weismannism.' Its purpose, as stated in the Preface, was not to discuss either the question of the inheritance of acquired characters, with which Professor Weismann's name is so closely associated, or the conception of the architecture of germplasm, which is no less essential to his general theory. What he had in view was rather to trace, for the information of non-biological readers, the successive stages through which that theory has passed during the half-dozen years that it has been before the reading public in England. Notwithstanding its polemical character, it contains much that is of value and interest, particularly the comparison between the theory of heredity, submitted to the Royal Society by Mr. Francis Galton in 1872, with the doctrine now advocated by the great German naturalist.

In conclusion, reference must be made to Romanes' numerous and valuable contributions to the study of comparative psychology. On this subject Professor Lloyd Morgan has been kind enough to furnish the writer with some valuable notes.

Romanes first became known as a writer on comparative psychology when he delivered, as already mentioned, one of the evening lectures on "Animal Intelligence," at the meeting of the British Association at Dublin in 1878. His first published work with the same title appeared in 1881. In it he set forth conclusions as to the mental endowments of animals, which were founded partly on his own observations, partly on those of other persons, which he had collected from a great variety of sources. This preparatory volume was followed in 1883 by a work on 'Mental Evolution in Animals,' in which he lucidly and brilliantly set forth what he regarded as the principles of the subject. Into this treatise were "woven" *as quotations* certain notes given to Romanes by Mr. Darwin during his life; besides which "a part of Mr. Darwin's chapter on Instinct, written for the 'Origin of Species,' but afterwards suppressed for the sake of condensation," was added as an appendix.

'Mental Evolution in Animals' was followed in 1888 by an instalment of a work on 'Mental Evolution in Man,' dealing with the origin of the human faculty. It was the last work of the Mental Evolution Series which the author was ever to complete; but it was his intention to widen the field of discussion at a later period. The book encountered its full share of criticism. The author's views were objected to, on the one hand, by those to whom the evolution of human from animal intelligence was unacceptable, on the other, by those who, on scientific grounds, were unable to accept Romanes' interpretation of animal faculty. All, however, would be willing to admit that, to quote the words of Professor Lloyd Morgan, "by his patient collection of data, by his careful discussion of these data in the light of principles clearly and definitely formulated, by his wide and forcible advocacy of his views, and above all by his own observations and experiments, Mr. Romanes left a mark in this field of investigation and interpretation which is not likely to be effaced."

In the preceding paragraphs an endeavour has been made to give a concise summary of Romanes' contributions to biological science.

Such an account would not, however, completely enable the reader to form a just conception of the influence which he exercised during the last ten years of his life, by his writings and public utterances. It would, indeed, be doing scant justice to his memory if, even in recording the dry additions to knowledge which resulted from his work, we were to forget what science owes to him for having awakened in the minds of thousands of readers, both in England and in America, that interest in the relations of man to his living surroundings, which is so often the first step in the training of a naturalist. All who know Romanes' writings will agree that, quite independently of the subjects to which they relate, they possess qualities which sufficiently account for the favour with which they have been received on both sides of the Atlantic. Even those who are at variance with Romanes on fundamental questions, are charmed by the unconstrained and natural way in which his originality shines out in his language, and reconciled, in spite of their convictions, by the directness and simplicity of his reasoning. A considerable share of the interest which the questions on which he wrote at present excite, may be directly traced to his writings, the popularity of which affords the best evidence of their adaptedness to their purpose.

One word as to Romanes' personal qualities. He had many opponents, no enemies. Transparently honest himself and scarcely knowing what bitterness meant, he looked for and found the same kindliness and honesty in others and thus often made friends of his antagonists. The wide range of his interests and the number of the subjects about which he was able to talk suggestively, made him a most attractive companion. He was courteous to all, but reserved the best of his gifts—his overflowing charity and ungrudging sympathy—for his intimate friends. To how many who stood in this relation to him is his loss an irreparable calamity!

J. B. S.

ALPHONSE DE CANDOLLE: When the history of the progress of botany during the 19th century shall be written, no names will be more conspicuous for the value and extent of their united labours than those of the de Candolle's, father and son, Auguste Pyrame and Alphonse. Their labours embraced every department of the science, morphological, physiological, systematic, geographical, and economic, and were uninterrupted during ninety-five years, those of the father extending from 1798 to 1842, and of the son from 1824 to 1893. And if the period during which their labours overlapped be taken into account, the total amounts to 113 years.

The subject of this notice, Alphonse Louis Pierre de Candolle, was born in Paris, October 27, 1806, and passed his earlier years at Montpellier, where his father, a Genevese, of Huguenot descent, and a

Protestant, held the professorship of botany. In 1814 the father was transferred to the botanical chair of Geneva, and there, in 1825, Alphonse took the degree of Bachelor of Science, and commenced the study of law. In 1829 he took his doctor's degree in that faculty, selecting as the subject of his inaugural thesis "Le Droit de Grâce." It should be observed, in passing, that his legal education had a marked influence on his subsequent career, both as a citizen and as a botanist; on the one hand it qualified him to undertake civic duties of inestimable advantage to his canton and country, and on the other it contributed largely to the development of those qualities of exactitude in collecting and recording facts, of methodical arrangement of these, and of cautious reasoning, that are predominant features in all his scientific writings.

From a very early age Alphonse de Candolle had developed a love of botany, and, when only 18 years old, he published his first contribution to this science, in a paper on *Agaricus tubæformis*,* a remarkable dimorphic fungus, found on the wood of thermal baths in Piedmont, which appeared in the 'Annales des Sciences Naturelles' (vol. 1, p. 347).

In 1830 he began to co-operate with his father in the elaboration of the colossal 'Prodromus Systematis Vegetabilium,' by the preparation of a monograph in advance of that work, on the *Campanulaceæ*, which at once raised him to a high position as a botanist. It was followed in 1832 by another monograph, supplemental to the 'Prodromus,' upon the very obscure and little known tropical family of *Anonaceæ*, which, owing to the want of materials, had been inefficiently dealt with in that work. On his father's death in 1841 the future conduct of the 'Prodromus' was undertaken by Alphonse, who for upwards of half a century zealously laboured towards its completion as editor and part author of the eight succeeding volumes; and thereafter of its continuation in the 'Monographiæ Phanerogamarum,' in which he was zealously aided by his eminent son Casimir.

Alphonse de Candolle's individual contributions to the 'Prodromus' consist of detailed descriptions of forty-five natural families, containing 438 genera and upwards of 5300 species. Amongst these the most important are the *Myrsinaceæ*, *Lobeliaceæ*, *Apocynaceæ*, *Myristicaceæ*, *Begoniaceæ*, and *Cupuliferæ*. The judgment of botanists upon these (and upon those not enumerated) is that they exhibit all the best qualities of monographic work, scientific and scholarly. As an example of the completeness of his work, the last mentioned order (*Cupuliferæ*) may be instanced. It includes the great genus of oaks, which had previously been represented descriptively by a chaos of ill-defined species of the north temperate zone, and little more than indications of the existence of a vast contingent of Oriental, Asiatic, and Malayan groups of species, specimens of which were scattered through various great herbaria of Europe. These last presented the greatest difficulties, and as showing how successfully they were treated, it is instructive to find that in Dr. King's very recent magnificent illustrated work on the oaks of India and the Malay Islands, and which is founded on materials immeasurably superior to what Alphonse de Candolle had access to, and on double the number of species, all but one-eighth of those described in the Prodromus are found to have been satisfactorily established upon sound scientific principles.

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* Some of the most eminent botanists commenced the study of plants with the lower cryptogams, as did the elder de Candolle, the first of whose long list of publications was on the nutrition of lichens.

Before concluding the subject of the Prodromus, it must be recorded that over and above the labour expended on his own contributions to it was that involved in the editorship of the 17 volumes, in the collection of materials for his collaborators, in the correspondence with the latter, and in the revision of their work in MS. and in the press—laborious and often irksome duties—all scrupulously performed.

The knowledge and experience acquired during the progress of the Prodromus led to an exposition, for the guidance of monographers, of his views as to the right method of proceeding in describing plants. Three treatises may be classed under this head. The first is a paper published in 1862, and entitled " Études sur l'espèce, à l'occasion d'une Révision de la famille des Cupulifères." It is an interesting and instructive study of the meaning and application of the term species as employed by naturalists, its limits and those of its members, in time and area, their origin, future, and modifications. For such a study he suggests the term epiontology.

The second is entitled "La Phytographie, ou l'art de décrire les végétaux considérés sous différents points de vue," published in 1880. Founded primarily on the principles so well laid down by Linnæus, this excellent work embodies the results of de Candolle's own long experience and those of his predecessors and contemporaries, on the methods that should be followed by biologists, whether botanists or zoologists, in describing organised beings. In this work the good effect of de Candolle's legal training is conspicuous, for the principles which he would see adopted, besides being unexceptionable, are laid down with judgment, precision, and moderation that leave nothing to be desired.

The third is his " Lois de la Nomenclature Botanique," which appeared in 1867, and in which rules were given for selecting, amongst conflicting claims and opinions, the name a genus or species should bear, assuming that it had borne two or more. So impressed was de Candolle with the importance of this subject, that it was always uppermost in his thoughts during forty years of his life, during which period he published twelve treatises on the subject. The result has been the general acceptance of the principles he laid down, and the adoption of the rules, except where expediency or exceptional conditions clearly require their modification or supersession.

Turning from A. de Candolle's taxonomic labours, it is on his 'Géographie Botanique Raisonnée,' published in 1858, that his most enduring claim, as a philosophical botanist, rests. The subject had early attracted his attention. He was drawn to it both by inclination and by his father's admirable 'Essai Élémentaire de Géographie Botanique' (published in 1820). An interesting chapter in his own monograph on *Campanulaceæ* (his earliest systematic work) is devoted to it; a more important one is embodied in his 'Introduction à l'Étude de la Botanique' (1835); and in 1848 he published ('Comptes Rendus') a suggestive essay on the northern limitation of plants, adopting as a leading principle the "Méthode des Sommes de Température," as laid down by Boussingault in his "Economie Rurale."

It is impossible in a notice like the present to give even a general review of the contents of a work covering such a wide range of interesting and important considerations as the 'Géographie Botanique Raisonnée,' and which enters into so many of these in extraordinary detail. The impressions left in the mind after a first perusal are of surprise at the astonishing number of the valuable observations collected and collated from apparently inexhaustible sources, the precision and methodical subdivision of the subject matter, and the scrupulous care with which the labours of every previous author and reasoner are treated. It may truly be said of the author of the 'Géographie Botanique,' as has been of Darwin's 'Origin of Species,' that he has utilised to the best purpose the waste and the rejected observations of scores of less sagacious reasoners. Throughout the work the search for general principles and laws is conspicuous. No methods known to him are left untried, the statistical above all others, and even when these all fail, the materials left for the analysis of future thinkers are so well knit together and so absolutely trustworthy that they have in numberless instances proved of great value. The most conspicuous instance of this is to be found in the exhaustive chapters devoted to the influences of heat and light on the development of individuals and of species, and on the distribution of the higher groups of plants. Though not always leading directly to a precise correlation of cause with effect in the desired direction, the value of these researches appears in another chapter, where it is shown by a reference to them that the limitation of species, whether on plains or mountains, is determined by the amount of heat above the freezing point of water, that is absorbed during the months occupied in growth, and not by the means or extremes of annual temperature. Influenced, no doubt, by his early legal studies, there is throughout the 'Géographie Botanique' a disregard, almost amounting to a negation, of speculative and theoretical considerations; but, under this point of view, it must be remembered that the 'Origin of Species' had not as yet appeared; that Forbes' and Lyell's pregnant suggestions, as affecting the dispersion of species, were not generally accepted, and that the doctrine of the multiple origination of individuals of a species was entertained, if not formally adopted, by de Candolle as the more probable theory of creation; as also that genera were considered by him to be more natural (better limited) groups than species.

In 1882 appeared the 'Origine des Plantes Cultivées,' an expansion of Chapter IX of the 'Géographie,' but which, being published thirty years later, and containing a vast amount of additional matter, is really a new and independent work. It is prefaced by two chapters, one on the supposed origin of the art of cultivation in different countries, the other on the methods he employed in tracing the sources whence cultivated plants have been obtained. These methods are botanical, archæological, palæontological, historical, and linguistic, in the application of all of which the author has shown great learning and judgment, and the result is a work of extraordinary interest and value. It has already gone through three editions, and a fourth is urgently needed, because many tropical cultivations have still to be dealt with, and because some very important supplemental facts, especially as regards plants of American origin, have been published in an elaborate review of de Candolle's work by Dr. A. Gray and J. H. Trumbull, in the 'American Journal of Arts and Sciences' (April—August, 1883).

Another work, though more of a literary than of a purely scientific character, must not be omitted in a review of de Candolle's labours: it is his 'Histoire des Sciences et des Savants depuis deux Siècles, précédée et suivie d'autres Études sur des sujets scientifiques, en particulier sur l'Hérédité et la Sélection dans l'espèce humaine," of which the first edition appeared in 1873, and a greatly enlarged one in 1885. It need not be said that the 'Origin of Species' suggested this fascinating work, and that the writings of Grove, Spencer, Galton, and Wallace have all been largely drawn upon in its execution. Both genius and originality are displayed in it, whether in respect of the problem it essays to solve or the views which it sets forth. With his undeviating preference for the statistical method, he enumerates by name all the scientific men who had attained the honour of Associates and Correspondents of the Academies of Science of Paris and Berlin, and of Foreign Fellowship of the Royal Society of London, between 1664 and 1882, with their nationalities and the branches of science for which they were distinguished. After subjecting the lists to a severe scrutiny, he finds that Switzerland carries away the palm, in respect of the number in proportion to its population, of the recipients of these academical honours. England he considers to be the country most favourably conditioned for intellectual culture, especially due to the facility with which its language may be acquired for reading purposes. This led to an analysis of the chief European languages, with the result that, owing to its "comparatively simple declensions and conjugations, its facility for forming compound words, and its abundance of short declamatory expressions, English is likely to become the universal language of the future."

By editing the autobiography of his distinguished father, A. de Candolle has enriched the history and literature of science. The father was a man of genius and varied attainments, a lover of society, and an excellent talker, who cultivated the acquaintance of the most intellectual people in France and England during the first forty years of the present century, and his autobiography contains instructive anecdotes, often racy and never injurious, of not a few of the most eminent men of his time.

A. de Candolle's professional career as a botanist began in 1831, when he was appointed Honorary Professor of Botany in the Academy of Geneva, assistant to his father as Director of the Botanical Gardens, and conductor of the students' herborizations. In 1841, on his father's death, he succeeded him in the chair, which he held till 1850. In 1835 he published, as a class book, his 'Introduction à l'Étude de la Botanique,' an excellent work, conceived and executed in the spirit of his father's 'Organographie Végétale.' It was reprinted in Belgium, and translated into German and Russian.

A. de Candolle's labours were not confined to botany, for throughout the active period of his life he was engaged in civic duties. As early as 1834 he was elected a representative of the Council of Geneva, later a member of the Constituent Assembly, and lastly of the Grand Council from 1862—1866. In all movements for the improvement of the city and well-being of his fellow-citizens and countrymen, especially the financial and charitable, he was a zealous and judicious advocate, and he published many well reasoned papers on these and kindred subjects. He was the first to advocate the Referendum in financial affairs, and the use of postage stamps in his canton, and subsequently in the Confederation. He was a zealous supporter of the Société de Physique et d'Histoire Naturelle de Genève, of the Société Helvétique des Sciences Naturelles, of the Société des Arts, and the Société de Lecture, and a perennial contributor to the 'Archives des Sciences Physiques et Naturelles.' Of separate publications and articles in scientific and other journals, M. Micheli* enumerates 235,

* 'Archives des Sciences Physiques et Naturelles,' Geneva, December, 1893.

of which seventy-eight are included in the Royal Society's 'Catalogue of Scientific Papers' up to 1883. These embrace an extraordinary variety of subjects, and it can truly be said that all are thoughtful and worthy of serious attention.

Personally Alphonse de Candolle was distinguished for courtesy and liberality in thought and deed. Ever helpful, no one applied to him in vain for aid. His vast stores of knowledge, his herbarium, his library, and even his roof were at the service of all who could make good use of them. The herbarium and library were left to his son Casimir, under the conditions that they be open to botanists, and that he conducts the publication of the 'Monographiæ Phanerogamarum' (continuation of the Prodromus). He married in 1832, Mademoiselle Jeanne Victoire Kunkler, who pre-deceased him, and by whom he had two sons. His residence was Geneva during the summer, and Valon, a few miles distant, in the winter, where he died 4th April, 1893, in his eighty-seventh year. Amongst the numerous honours conferred upon him was that of Correspondent of the Institute of France, of which body he was afterwards elected one of the eight Foreign Associates in succession to Agassiz. He was a Foreign Fellow of the Royal and Linnean Societies of London, and of most European and American academies, a Doctor of Laws of Oxford and Cambridge, and the recipient of various medals and orders, including the gold medal of the Linnean Society, and last, but not least, the Prussian order of "Pour le Mérite."

J. D. H

PIERRE JOSEPH VAN BENEDEN was born at Malines on the 19th of December, 1809, and died at Louvain on the 8th of January, 1894. His long life is one record of indefatigable industry in the pursuit of his favourite science, but otherwise was singularly uneventful. He received a medical education, and almost immediately after taking his degree, was, in 1831, appointed curator of the Natural History Museum of the University of Louvain, and in 1836, Professor of Zoology and Comparative Anatomy in the Catholic University founded in the same town, after the suppression of the ancient State University. He continued steadily to discharge the duties of this office, combined with an uninterrupted course of scientific research, until within a few days of his death, which resulted from a somewhat sudden attack of bronchitis. He rarely quitted his home except for the purpose of visiting museums, or with some other scientific object in view.

The jubilee of his professoriate was celebrated with great honour, both at Louvain and at his native town, in 1886, seven years before the termination of his career.

His earliest scientific work was devoted to the anatomy and development of the Mollusca, the first published memoir from his pen, which appeared in the "Annales des Science Naturelles," for 1835, being upon the genus *Dreissena*. Numerous other papers followed one another in rapid succession, chiefly in the "Mémoires" or "Bulletins" of the Belgian Academy, and his researches soon extended into other branches of zoology, but mainly the aquatic, and especially marine forms of invertebrates (as Hydrozoa, Bryozoa, Turbellaria, Ascidians, Archiannelides, Crustacea, Hydrachnida, and Pentastomida), which frequent residences at Ostend during his academic vacations gave him opportunities of observing. For this purpose he established at his own expense in 1843 a marine laboratory and aquarium, which was one of the first, if not quite the first, of these now familiar institutions. He also greatly advanced the knowledge of the development, transformations and life-histories of the parasitic worms, and their relations to their respective hosts, one of his principal memoirs on this subject obtaining in 1858 the "*grand prix des sciences physiques*" of the Institute of France. A summary of his researches into a branch of zoology which he made peculiarly his own, was published in a popular form, as one of the International Scientific Series, under the title of "*Les commensaux et les parasites dans le règne animal,*" 1875. Of this, English and German translations were issued.

It is singular that throughout nearly the whole of his long and industrious scientific career he was engaged more or less in two quite independent and very distinct lines of work, the one which has been referred to above, and another by which to many he was almost exclusively known, and which in his later days became of such absorbing interest as to give him little leisure for any other. Very early in life he was attracted by the discoveries of fossil bones of whales made during the excavations carried on in the formation of the fortifications at Antwerp, and this led to a systematic study of this group of mammals, then, owing to the want of materials in museums, most imperfectly known. With an extraordinary pertinacity and zeal, which was continued to the end of his life, he gathered from all available sources everything that could be known of the Cetacea, both existing and extinct, and published the knowledge he acquired in innumerable short papers and memoirs, and in several great and handsomely illustrated works, of which one, the "*Ostéographie des Cétacés vivants et fossiles,*" 1868–80 (written in conjunction with Paul Gervais), will long remain the standard work of reference on the subject. Others especially relate to the extinct species found in the neighbourhood of Antwerp, and are included in a series, which also contains a description of the fossil seals from the same locality, published in the "*Annales du Musée Royal d'Histoire Naturelle de Bruxelles.*"

Although it was chiefly among the lower forms of animal life and the Cetacea that Van Beneden came before the world as an original observer, a glance at the long list of his published memoirs (more than two hundred in number) show that subjects from various other groups occasionally engaged his attention and his pen, and he was also the joint author, with Paul Gervais, of a general work, in two volumes, on Medical Zoology, published in 1859.

Any notice of Van Beneden would be incomplete without reference to his high character and remarkably courteous and agreeable manners. He was gentle, modest, kind and considerate to others, and much beloved by all who knew him intimately, as the writer of this notice had many opportunities of observing, both in his own family circle at Louvain and on many visits which he paid to England, during which he was always a most welcome guest. Though he remained to the end a devoted son of the Church in which he had been brought up, he always showed the widest toleration for the views of others. The meetings of the British Association had a special attraction for him, and, more than once, he brought original communications before them. His last visit to this country was on the occasion of the tercentenary celebration of the University of Edinburgh in 1884, when he was the recipient of the honorary degree of LL.D. He was elected a Foreign Member of the Royal Society in 1875, and was also on the list of foreign members of the Linnean, Geological, and Zoological Societies of London. He was President of the Royal Belgian Academy in 1881, and on the occasion of his jubilee in 1886, was nominated Grand Officer of the Order of Leopold. He has left a large family of daughters and one son, Edward, Professor of Zoology in the University of Liege, who has already acquired a reputation in science which bids fair to equal, if not surpass, that of his father.

W. H. F.

ROBERT YOUNG ARMSTRONG* was the youngest son of the Rev. William Armstrong, incumbent of Calry, co. Sligo, Ireland, by his wife, Cassandra Maria, daughter of John Young, of Castlerea, Roscommon, nephew of Dr. Matthew Young, F.T.C.D., Bishop of Clonfert.

He was born at Calry Glebe the 19th September, 1839. His early education was conducted at the Clergy Sons' Institute, at Lucan, and, later, at the Academic Institute, Harcourt Street, Dublin. He entered Trinity College, Dublin, in 1856, and finally obtained a commission in the Royal Engineers on the 21st December, 1858.

After being engaged for a few years in the construction of Fort Burgoyne, at Dover, he was sent in 1863 to Canada, where he was stationed first at Montreal, and afterwards at Hamilton, on Lake Ontario. Returning to England in 1868, he was appointed Assistant-Instructor in Signalling at Chatham. His appointment was changed in 1871 to that of Assistant-Instructor in Telegraphy. General electricity and submarine mining were included in his educational duties, which he carried out with energy and enthusiasm. He liked the work, and, without doubt, he was specially fitted for it. During the succeeding twelve years, under his hands and those of his colleagues, the application of electrical science to military engineering received extraordinary development. He became a Captain on the 3rd August, 1872, and four years later succeeded Colonel Malcolm, R.E., as Instructor in Telegraphy, an important post which he held for nearly seven years.

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* I am indebted to the 'Royal Engineers Journal' for the details of Colonel Armstrong's career.

To properly appreciate Armstrong's work during these years, it must be remembered that electrical science was comparatively in a backward condition. The instruments in use in the military telegraph service were obsolete. The methods of exploding charges for demolitions and for land mines were crude in design and uncertain in action. The use of the arc light for purposes of coast defence had scarcely been thought of, although the general subject of defence had been for some years attracting attention, and the desirability of employing booms and submarine explosives for the protection of ports had been considered. As the result of an exhaustive mintue by Sir John Burgoyne, the Floating Obstruction Committee carried out a number of experiments, and finally decided to introduce the principle of submarine mining. The organisation of the first submarine mining company fell to the lot of Armstrong and his staff at Chatham. Powers of invention, energy, and zeal were requisite, and fortunately for the public service he possessed them all.

The records of the War Office Torpedo Committee, 1871-1876, and the Proceedings of the Royal Engineers Committee, show clearly the important nature of his work in connection with the carefully-conducted experiments that were necessary in order that the apparatus might be brought to a high state of efficiency. He introduced the system of testing submarine mines by means of relays.

In 1879 he, with others, carried out a series of experiments preparatory to the employment of the arc light for coast defence.

He became a Major on the 9th October, 1879, and in January, 1883, he was appointed Electrical Adviser to the Board of Trade. The introduction of electric lighting made it necessary to pass an Act of Parliament. He advised the Board on the difficult points that arose in connection with the efficiency of supply and the public safety.

He succeeded Colonel Malcolm as Inspector of Submarine Defences at the War Office in June, 1884, and continued also to advise the

Board of Trade. However, in 1888, by reason of the increased vitality exhibited by electric lighting companies, he found his duties becoming so onerous that he resigned his post at the Board of Trade, and devoted the whole of his attention to the War Office. He received the thanks of the Board for the valuable services he had rendered the department.

He gave most arduous work to the organisation, both in personnel and matériel, of the submarine defences. He served on many committees appointed to report upon special questions of defences. He represented the War Department at many electrical exhibitions at home and in foreign countries.

He was elected a Fellow of the Royal Society in 1891.

On the 1st July, 1890, he had been promoted to the rank of Colonel in the army; and on the 30th May, 1891, had been gazetted a civil C.B.

On the 1st July, 1891, he was appointed Royal Engineer member of the Ordnance Committee. His health, however, failed, and he retired on a pension on the 7th December, 1892. At first, the rest appeared to act beneficially, but early in 1894 he caught a chill, from which his delicate constitution never recovered. He gradually became weaker, and passed away on 1st November, 1894, at Prospect House, Omeath, Ireland.

He was buried at Mount Jerome Cemetery, Dublin.

Armstrong was a man of high personal character, of affectionate disposition and gentle manners, and the most pleasant of companions. He was greatly beloved in his own corps, in which he had numerous friends, and also by those with whom he was associated in the many important posts he held. By them he is deeply mourned.

P. A. M.

BISSET HAWKINS was born in London, in the year 1796, and was at the time of his death, on December 7, 1894, the oldest Fellow of our Society, having been elected on December 18, 1834, as well as by some fourteen years the oldest Fellow of the Royal College of Physicians.

Dr. Hawkins' place in the history of science and of medicine is as a student of public medicine, at a time when sanitary authorities and general register offices had not been devised. Returning to London, after his graduation at Oxford in 1825, he presently took an interest in medical statistics, a subject then in its infancy, and in a volume enlarging his Gulstonian Lectures, of 1828, he brought together what records of disease and death were obtainable about various communities, and found in them—to use his own words—proof of the empire of human art over disease. In 1831, when *cholera* was threatening St. Petersburg, he made a serviceable collection of facts, so far as they could be learned, respecting the march of the disease from India through Russia, and was able to end his story by telling of a Board of Health established for England, with Sir Henry Halford, President of the College of Physicians, at its head.

Dr. Hawkins' professional knowledge, combined with his regard for numerical accuracy, was recognised by his appointment successively to office as Factory Commissioner and Inspector of Prisons. He rendered useful service in these capacities, particularly in pleading for abridgment of extravagant hours of labour by women and children, and by preventing serious sanitary mistakes by the Government of the time in its dealings with prisoners. He afterwards became a Commissioner in Lunacy.

When, in 1836, provision was made for registering births and deaths throughout England, Dr. Hawkins was in a position—in the counsels of the College of Physicians and of the Government—to speak with authority in favour of a proposal to record the fatal disease in the death register; along with the mere facts of identity and of time and place. Of course it could not be long before the value of this record was felt; and the column—at first reserved experimentally for "causes of death"—has long been seen to be the most important of all the columns of the register for the purposes of the community.

Dr. Bisset Hawkins, in 1838, urging on England a greater interest than she then took in the affairs of Germany "and her allied countries," had to coin a phrase to distinguish the wider idea of social life taken in Germany from the subjects which formed the political economy of England at that period. He would have Englishmen study the "state economy" of Germany, her natural history, her educational, medical, and other concerns, that were at the beginning of the century much better cared for in Germany than in his own country. He lived to see his distinction well nigh effaced; and we may credit him with having helped to the effacement.

He spent the later years of his life at his residence at Bournemouth, in Dorsetshire, a deputy-lieutenant of the county, the ideal of an English country gentleman and retired scholar.

G. B.

Albert William Beetham was born in London, on August 20, 1802, being the eldest son of William Beetham, F.R.S., Deputy-Lieutenant for the county of Middlesex. He was elected a Fellow of the Linnean Society on February 6, 1827, and of the Royal Society on February 5, 1835. He was appointed Clerk of the Cheque and Adjutant of the Honourable Corps of Gentlemen-at-Arms on July 3, 1835. In November, 1836, he was called to the Bar at Gray's Inn, and practised as a special pleader on the Western Circuit. He was appointed Recorder of Dartmouth in 1860, and at the time of his death was Deputy-Lieutenant for the Tower Hamlets, and Justice of the Peace for Middlesex and the Tower Hamlets. He married, in 1827, Caroline Jane, daughter of John Caplin, Master of the Leathersellers' Company, of the City of London, and died at West Cliff, Dawlish, Devonshire, on March 11, 1895.

M. F.

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ERRATA.

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Page 92, 8th line from top, § 10, *for* "no" ***substitute*** "comparatively little."
„ 93, last line but one of § 13, *for* "6.10^{-8}" ***substitute*** "6.10^{-11}," and *for* "4·8 per cent." ***substitute*** "$4{\cdot}8.10^{-8}$."
„ „ delete the first paragraph of § 14.
„ „ line 5 from the end of § 14, *for* "Even" ***substitute*** "But," and in line 3 from the end of § 14 *for* "sufficient" ***substitute*** "insufficient."

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P. 267, line 12, *for* deduction *read* correction (additive).
„ „ 16, *for* 1·2505 *read* 1·2511.
„ „ 20, *for* 2·2984 *read* 2·2996.
„ „ „ *for* 0·87471 *read* 0·87517.
„ „ „ *for* 13·9954 *read* 14·003.

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